PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCIII.

PART I.

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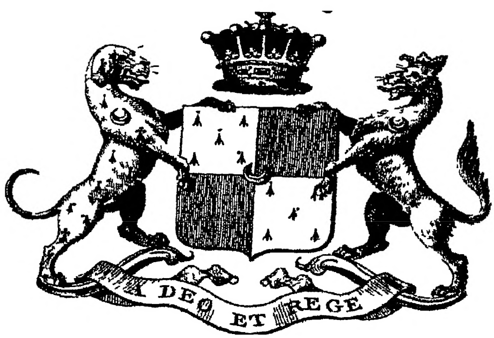
MDCCCIII.


A DEO ET REGE

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the year 1802, the Medal on Sir GODFREY COPLEY's Donation, to WILLIAM HYDE WOLLASTON, M. D. for his various Papers printed in the Philosophical Transactions.

And they adjudged the Gold and Silver Medals on the Donation of BENJAMIN COUNT OF RUMFORD, to COUNT RUMFORD himself, for his various Discoveries respecting Heat and Light.

PHILOSOPHICAL

TRANSACTIONS.

I. *The* Bakerian *Lecture. Observations on the Quantity of horizontal Refraction; with a Method of measuring the Dip at Sea. By* William Hyde Wollaston, *M. D. F. R. S.*

Read November 11, 1802.

In a Paper which I some time since presented to this Society, (printed in the Phil. Trans. for 1800,) I endeavoured to ascertain the causes, and to explain the various cases, of horizontal refraction, which I had either observed myself, or had seen described by others.

At the time of writing that essay, I had not met with the *Mémoires sur l'Egypte*, published but a short time before; and I was not aware that an account had been given by M. Monge, of the phenomenon known to the French by the name of *mirage*, which their army had daily opportunities of seeing, in their march through the deserts of Egypt.

In the perusal of this memoir, I could not fail to derive instruction from the information it contained; but, as the facts related by him agreed entirely with the theory that I had advanced, I was by no means induced to adopt the explanation that he has [illegible] in preference to my own.

The definite reflecting surface which he supposes to take place between two strata of air of different density, is by no means consistent with that continued ascent of rarefied air which he himself admits; and the explanation founded on this hypothesis will not apply to other cases, which may all be satisfactorily accounted for, upon the supposition of a gradual change of density, and successive curvature of the rays of light by refraction.

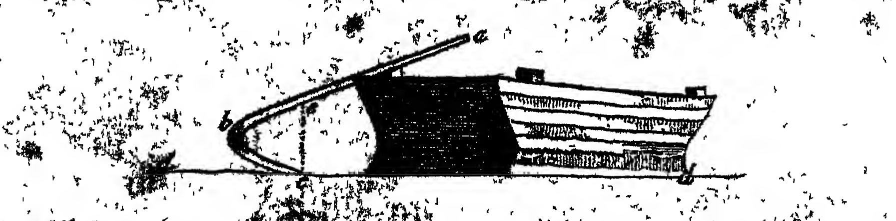
I have since learned that the same subject had also been ably treated by Mr. WOLTMAN, in GILBERT'S *Annalen der Physik;* but I have to regret that his dissertation, as well as that of GRUBER, in the same Annals, were written in a language that was unknown to me, and that I could not avail myself of the assistance that I might otherwise have received from their researches.

When I formerly engaged in this inquiry, being impressed with the advantage to be derived from it to nautical astronomy, on account of the variations in the dip of the apparent horizon, from which all observations of altitude at sea must necessarily be taken, I suggested the expediency of a series of observations, to be made by a person attentive to those changes of temperature or moisture of the atmosphere, on which he might find the depression of his horizon principally to depend. I had at that time no expectation that I could myself pursue this subject farther to any useful purpose, having little prospect of residing for a sufficient length of time in view of the sea, and seeing no other method by which the same [illegible] accomplished. I have, however, since that time, fo[illegible] myself by observations over the surface of the Th[illegible] although the quantity of refraction varies [illegible] change of

the thermometer or hygrometer, yet the law of these variations is not altogether so simple as I had hoped it might be found.

I shall, on the present occasion, first relate the facts on which this opinion is founded, and which are in themselves sufficiently remarkable, on account of the unexpected quantity of refraction observable over a short extent of water; I shall, in the next place, shew that the exact determination of the concurrent changes of the atmosphere are of less value, and their irregularities of less consequence, than I had conceived, as there is a very easy method whereby the quantity of dip at sea may be at any time correctly measured; and therefore the end which I sought by indirect means, may be at once directly attained.

The first instance that occurred to me, of observable refraction over the surface of the Thames, was wholly accidental. I was sitting in a boat near Chelsea, in such a position that my eye was elevated about half a yard from the surface of the water, and had a view over its surface, that probably somewhat exceeded a mile in length, when I remarked that the oars of several barges at a distance, that were then coming up with the tide, appeared bent in various degrees, according to their distance from me. The most distant appeared nearly in the form



here represented; *cd* being my visible horizon by apparent curvature of the water; *ab* the oar itself in its inclined position; and *bc* an inverted image of the portion *be*. By a little attention to other boats, and to buildings on shore, I could discern that the

appearance of all distant objects seen near the surface of the water was affected in a similar manner, but that scarcely any of them afforded images so perfectly distinct as the oblique line of an oar dipped in the water.

A person present at the time (as well as some others to whom I have since related the circumstance) was inclined to attribute the appearance to reflection from the surface of the water; but, by a moderate share of attention, a very evident difference may be discovered between the inversion occasioned by reflection, and that which is caused by atmospherical refraction. In cases of reflection, the angles between the object and image are sharp, the line of contact between them straight and well defined, but the lower part of the image indefinite and confused, by means of any slight undulation of the water. But, when the images are caused by refraction, the confines of the object and its inverted image are rounded and indistinct, and the lower edge of the image is terminated by a straight line at the surface of the water. In addition to these marks of difference, there is another circumstance which, if attended to, must at once remove all doubt; for, by bringing the line of sight near to the surface of the water, boats and other small objects are found to be completely hidden by an apparent horizon, which, in so short a distance, cannot be owing to any real curvature of the water, and can arise solely from the bending of the rays by refraction.

When I reflected upon the causes which were probably instrumental in the production of these phenomena, they appeared referrible to difference of temperature alone. After a succession of weather so hot that the thermometer, during one month preceding, had been 12 times above 80°, and on an average of

the month at 68°, the evening of that day (August 22, 1800) was unusually cold, the thermometer being 55°. The water might be supposed to retain the temperature it had acquired during a few weeks preceding, and, by warming the stratum of air immediately contiguous to it, might cause a diminution of its refractive density, sufficient to effect this inverted curvature of the rays of light, in the manner formerly explained. As I was at that time unprovided with instruments of any kind, I had it not in my power to estimate the quantity of refraction, or temperatures: and can only say that, to my hand, the water felt in an uncommon degree warmer than the air.

Being thus furnished with an unexpected field for observation, I from that time took such opportunities as similar changes of the weather afforded me, of examining and measuring the quantities of refraction that might be discovered by the same means over another part of the river, that I found most suited to my convenience.

The situation from which the greater part of my observations were made, was at the SE corner of Somerset house. The view from this spot extends under Blackfriars bridge, towards London bridge, upwards of a mile in length, and in the opposite direction through Westminster bridge, which is three quarters of a mile distant.

Such distances are however by no means necessary; and indeed the air over the river, in cold weather, is generally, or at least very frequently, not sufficiently clear for seeing distinctly to so great distances. For, since the winds which are most likely to effect a sufficient change of temperature, on account of their coldness, are usually from the E, or NE, the principal smoke of the town is then brought in that direction, and hovers, like a dense fog

over the course of the river. This circumstance deprived me of many opportunities which the changes of the thermometer indicated to be favourable for my purpose, and obliged me often to make use of shorter distances than I should otherwise have chosen, by bringing the line of sight as near as I could to the surface of the water.

For this purpose, I had a plane reflector fitted to the object-end of a small pocket telescope, at an angle of 45°, so that, when the telescope was held vertically, it gave a horizontal view at any level that was found most eligible. When the water has been calm, I have observed that the greatest refraction was visible within an inch or two of its surface, and I have then seen a refraction of six or seven minutes in the space of 300 or 400 yards: at other times, I have found it greatest at the height of a foot or two; but, in this case, a far more extensive view becomes necessary.

The first measures that I took were on the 23d of September, 1800. The water was $2\frac{1}{2}°$ warmer than the air, and I found a refraction of about 4′.

Oct. 17. The difference of temperature was 3°, and the refraction 3′.

Oct. 22. The water was $11\frac{1}{2}°$ warmer than the air, yet the quantity of refraction did not exceed 3′.

The smallness of the quantity of refraction upon this occasion, I attributed to the dryness of the atmosphere; conjecturing that a rapid evaporation might in great measure counteract that warmth which the water would otherwise have communicated to the air.

From that time, therefore, I have noted not only the heights of the thermometer in the water and in the air, but have added

also the degrees of cold produced by keeping the bulb of it moistened for a sufficient time to render it stationary. In confirmation of my conjecture respecting the dryness of Oct. 22, I have also, in the following Table, which comprises the whole of my observations, inserted a column from the Register kept at the apartments of the Royal Society, containing the heights of the hygrometer, on those mornings when my observations were made.

TABLE.

At 8, A. M.	Air.	Water.	Difference.	Refraction.	Cold by evaporation.	Hygrometer.
1800. Sept. 23	57	$60\frac{1}{2}^{\circ}$	$3\frac{1}{2}^{\circ}$	4′	– –	72°
Oct. 17	$46\frac{1}{2}$	$49\frac{1}{2}$	3	3	– –	72
22	38	$49\frac{1}{2}$	$11\frac{1}{2}$	3	– –	67
Nov. 1	41	$45\frac{1}{2}$	$4\frac{1}{2}$	8	$\frac{1}{2}^{\circ}$	76
4	$43\frac{3}{4}$	$46\frac{3}{4}$	3	3 –	$1\frac{3}{4}$	72
5	37	45	8	8 +	1	69
12	$44\frac{1}{2}$	$48\frac{1}{2}$	4	1 +	$3\frac{1}{2}$	73
13	40	$44\frac{1}{2}$	$4\frac{1}{2}$	5	$\frac{1}{2}$	76
1801. June 13	50	63	13	9 +	5	65
22	55	61	6	6 +	6	65
23	55	62	7	6	$4\frac{1}{2}$	65
24	55	61	6	5	3	67
Sept. 8	60	64	4	7	2	78
9	64	$64\frac{3}{4}$	$\frac{3}{4}$	5	3	74
10	58	64	6	7	2	70
12 o'clock, 10	63	64	1	2		

From a review of the preceding Table it will be found, upon the whole, that when the water is warmer than the air, some increase of depression of the horizon may be expected; but

that its quantity will be greatly influenced, and in general diminished, by dryness of the atmosphere.

It appears, however, that no observable regularity is deducible from the measures above given; but that the quantity, on some occasions, is far different from what the states of the thermometer and hygrometer would indicate. On the 9th of September, for instance, the difference of temperature is only $\frac{3}{4}°$, and the evaporation, to counteract this slight excess of warmth, produced as much as 3° of cold; nevertheless, the refraction visible was full 5′. In this observation I think that I could not be mistaken, as the water was at the time perfectly calm, the air uncommonly clear, and I had leisure to pay particular attention to so unforeseen an occurrence.

This one instance appears conformable to the opinion entertained by Mr. HUDDART, and by M. MONGE, that, under some circumstances, the solution of water in the atmosphere causes a decrease in its refractive power; but, on no other occasion have I been induced to draw a similar inference.

The object that I have at all times chosen, as shewing best the quantity of refraction, has been either an oar dipped in the water at the greatest discernible distance, or some other line equally inclined; and the angle measured has been, from the point where the inverted image is terminated by the water, to that part of the oar itself which appears to be directly above it. (The apparent magnitude of *ec*, Fig. p. 3.)

The eight first angles were taken with a mother-of-pearl micrometer in the principal focus of my telescope, and are not so much to be depended upon for accuracy as the succeeding eight. These last were measured with a divided eye-glass

micrometer, and consequently are not liable to any error from unsteadiness of the instrument or object.

From the foregoing observations we learn, that the quantity of refraction over the surface of water may be very considerable, where the land is near enough to influence the temperature of the air. At sea, however, so great differences of temperature cannot be expected; and the increase of dip caused by this variation of horizontal refraction, it is to be presumed, is not so great as in the confined course of a river; but, if we consider that it may also be subject to an equal diminution from an opposite cause, and that the horizon may even become apparently elevated, there can be no question that the error in nautical observations, arising from a supposition that it is invariably according to the height of the observer, stands in need of correction.

The remedy employed by Mr. HUDDART,* of taking two angles of the sun from opposite points of the horizon at the same time, and considering the excess of their sum above 180° as double the dip, must without doubt be effectual; but, from causes which he assigns, it is practicable only within certain limits of zenith distance; for, where the zenith distance is small, and the changes of azimuth rapid, there is required considerable dexterity and steadiness of a single observer who attempts to turn in due time, from one observation to another; and, when it exceeds 30°, the greater angle cannot be measured with a sextant, and consequently his method is, with that instrument, of use only in low latitudes.

On account of the difficulty attending some of the adjustments for the back observation, he rejects that method for

* Phil. Trans. for 1797, p. 49.

taking angles in general, with much reason; but he has thereby overlooked a means of determining the dip, which I am inclined to think might be employed with advantage in all latitudes, without any occasion to hurry the most inexperienced or cautious observer.

By the back observation, the whole vertical angle between any two opposite points of the horizon may be measured at once, either before or after taking an altitude. Half the excess of this angle above 180°, should of course be the dip required.

But, if it be doubtful whether the instrument is duly adjusted, a second observation becomes necessary. The instrument must be reversed, and, if the apparent deficiency of the opposite angle from 180° be not equal to the excess before obtained, the index error may then be corrected accordingly; and, since the want of adjustment, either of the glasses at right angles to the plane of the instrument, or of the line of sight parallel to it, will affect both the larger and smaller angle very nearly in an equal degree, the $\frac{1}{4}$ part of their difference will be extremely near the truth, and the errors arising from want of those adjustments may with safety be neglected.

This method of correcting the index error for the back observation at sea, was many years since recommended by Mr. LUDLAM;* yet I do not find that it has been noticed by subsequent writers on that subject, or suggested by any one for determining the dip; but I can discover no reason for which it could be rejected as fallacious, and I should hope that in practice it would be found convenient, since in theory it appears to be effectual.

The most obvious objection to this, as well as to Mr. HUDDART'S

* Directions for the Use of HADLEY'S Quadrant, 1771. § 82, p. 56.

method, is the possibility that the refraction may be in some measure different in opposite points of the horizon at the same time. When land is at no great distance, such an inequality may be found to occur; but, upon the surface of the ocean in general, any partial variations of temperature can rarely be supposed to exist; and it is probable, that under any circumstances, the difference will not bear any considerable proportion to the whole refraction; nor can it be thought a sufficient reason for rejecting one correction proposed, that there may yet remain other smaller errors, to which all methods are equally liable, but which it is not the object of the present dissertation to rectify.

II. *A chemical Analysis of some Calamines. By* James Smithson, *Esq. F. R. S.*

Read November 18, 1802.

NOTWITHSTANDING the experiments of BERGMAN and others, on those ores of zinc which are called calamine, much uncertainty still subsisted on the subject of them. Their constitution was far from decided, nor was it ever determined whether all calamines were of the same species, or whether there were several kinds of them.

The Abbé HAUY, so justly celebrated for his great knowledge in crystallography and mineralogy, has adhered, in his late work,* to the opinions he had before advanced,† that calamines were all of one species, and contained no carbonic acid, being a simple calx of zinc, attributing the effervescence which he found some of them to produce with acids, to an accidental admixture of carbonate of lime.

The following experiments were made to obtain a more certain knowledge of these ores; and their results will show the necessity there was for their farther investigation, and how wide from the truth have been the opinions adopted concerning them.

Calamine from Bleyberg.

a. The specimen which furnished the subject of this article,

* *Traité de Mineralogie,* Tome IV.

† *Journal des Mines.*

was said by the German of whom it was purchased, to have come from the mines of Bleyberg in Carinthia.

It was in the form of a sheet stalactite, spread over small fragments of limestone. Its texture was not however at all crystalline, but of the dull earthy appearance of chalk, though, on comparison, of a finer grain and closer texture.

It was quite white, perfectly opaque, and adhered to the tongue; 68.0 grs. of it, in small bits, immersed in distilled water, absorbed 19.8 grs. of it, = 0,29.

It admitted of being scraped by the nail, though with some difficulty: scraped with a knife, it afforded no light.

68.1 grs. of it, broken into small pieces, expelled 19.0 grs. of distilled water from a stopple bottle. Hence its density = 3.584. In another trial, 18.96 grs. at a heat of 65° FAHRENHEIT, displaced 5.27 grs. of distilled water; hence the density = 3.598. The bits, in both cases, were entirely penetrated with water.

b. Subjected to the action of the blowpipe on the coal, it be came yellow the moment it was heated, but recovered its pristine whiteness on being let cool. This quality, of temporarily changing their colour by heat, is common to most, if not all, metallic oxides; the white growing yellow, the yellow red, the red black.

Urged with the blue flame, it became extremely friable; spread yellow flowers on the coal; and, on continuing the fire no very long time, entirely exhaled. If the flame was directed against the flowers, which had settled on the coal, they shone with a vivid light. A bit fixed to the end of a slip of glass, wasted nearly as quickly as on the coal.

It dissolved in borax and microcosmic salt, with a slight

effervescence, and yielded clear colourless glasses; but which became opaque on cooling, if over saturated. Carbonate of soda had not any action on it.

c. 68.0 grs. of this calamine dissolved in dilute vitriolic acid with a brisk effervescence, and emitted 9.2 grs. of carbonic acid. The solution was white and turbid, and on standing deposited a white powder, which, collected on a small filter of gauze paper, and well edulcorated and let dry, weighed only 0.86 gr. This sediment, tried at the blowpipe, melted first into an opaque white matter, and then partially reduced into lead. It was therefore, probably, a mixture of vitriol of lead and vitriol of lime.

The filtered solution, gently exhaled to dryness, and kept over a spirit-lamp till the water of crystallization of the salt and all superfluous vitriolic acid were driven off, afforded 96.7 grs. of perfectly dry, or *arid*,* white salt. On re-solution in water, and crystallization, this saline matter proved to be wholly vitriol of zinc, excepting an inappretiable quantity of vitriol of lime in capillary crystals, due, without doubt, to a slight and accidental admixture of some portion of the calcareous fragments on which this calamine had been deposited. Pure martial prussiate of tartar, threw down a white precipitate from the solution of this salt.

In another experiment, 20.0 grs. of this calamine afforded 28.7 grs. of arid vitriol of zinc.

d. 10 grs. of this calamine were dissolved in pure marine acid, with heat. On cooling, small capillary crystals of muriate of lead formed in the solution. This solution was precipitated

* *Dry*, as opposed to wet or damp, which are only degrees of each other, merely implies free from mechanically admixed water. *Arid*, may be appropriated to express the state of being devoid of combined water.

by carbonate of soda, and the filtered liquor let exhale slowly in the air; but it furnished only crystals of muriate of soda.

e. 10 grs. dissolved in acetous acid without leaving any residuum. By gentle evaporation, 20.3 grs. = 2.03, of acetite of zinc, in the usual hexagonal plates, were obtained. These crystals were permanent in the air, and no other kind of salt could be perceived amongst them.

Neither solution of vitriolated tartar, nor vitriolic acid, occasioned the slightest turbidness in the solution of these crystals, either immediately or on standing; a proof that the quantity of lime and lead in this solution, if any, was excessively minute.

f. A bit of this calamine, weighing 20.6 grs. being made red hot in a covered tobacco-pipe, became very brittle, dividing on the slightest touch into prisms, like those of starch, and lost 5.9 grs. of its weight = 0.286. After this, it dissolved slowly and difficultly in vitriolic acid, without any effervescence.

According to these experiments, this calamine consists of,

Calx of zinc - - -	0.714
Carbonic acid - - -	0.135
Water - - - -	0.151
	1.000.

The carbonates of lime and lead in it are mere accidental admixtures, and in too small quantity to deserve notice.

Calamine from Somersetshire.

a. This calamine came from Mendip Hills in Somersetshire.

It had a mammillated form; was of a dense crystalline texture; semitransparent at its edges, and in its small fragments; and upon the whole very similar, in its general appearance, to calcedony.

It was tinged, exteriorly, brown; but its interior colour was a greenish yellow.

It had considerable hardness; it admitted however of being scraped by a knife to a white powder.

56.8 grs. of it displaced 13.1 grs. of water, at a temperature of 65° FAHRENHEIT. Hence its density = 4.336.

b. Exposed to the blowpipe, it became opaque, more yellow, and friable; spread flowers on the coal, and consequently volatilized, but not with the rapidity of the foregoing kind from Bleyberg.

It dissolved in borax and microcosmic salt, with effervescence, yielding colourless glasses. Carbonate of soda had no action on it.

c. It dissolved in vitriolic acid with a brisk effervescence; and 67.9 grs. of it emitted 24.5 grs. = 0.360, of carbonic acid. This solution was colourless; and no residuum was left. By evaporation, it afforded only vitriol of zinc, in pure limpid crystals.

d. 23.0 grs. in small bits, made red hot in a covered tobacco-pipe, lost 8.1 grs. = 0.352. It then dissolved slowly and difficultly in vitriolic acid, without any emission of carbonic acid; and, on gently exhaling the solution, and heating the salt obtained, till the expulsion of all superabundant vitriolic acid and all water, 29.8 grs. of arid vitriol of zinc were obtained. This dry salt was wholly soluble again in water; and solution of pure martial prussiate of soda occasioned a white precipitate in it.

This calamine hence consists of,

Carbonic acid - - -	0.352
Calx of zinc - - -	0.648
	1.000.

Calamine from Derbyshire.

a. This calamine consisted of a number of small crystals, about the size of tobacco-seeds, of a pale yellow colour, which appeared, from the shape of the mass of them, to have been deposited on the surface of crystals of carbonate of lime, of the form of Fig. 28. Plate IV. of the *Cristallographie* of ROME' DE L'ISLE.

The smallness of these calamine crystals, and a want of sharpness, rendered it impossible to determine their form with certainty; they were evidently, however, rhomboids, whose faces were very nearly, if not quite, rectangular, and which were incomplete along their six intermediate edges, apparently like Fig. 78. Plate IV. of ROME' DE L'ISLE.

22.1 grs. of these crystals, at a heat of 57° FAHRENHEIT, displaced 5.1 grs. of water, which gives their density = 4.333.

Heat did not excite any electricity in these crystals.

b. Before the blowpipe, they grew more yellow and opaque, and spread flowers on the coal. They dissolved wholly in borax and microcosmic salt, with effervescence.

c. 22.0 grs. during their solution in vitriolic acid, effervesced, and lost 7.8 grs. of carbonic acid = 0.354. This solution was colourless, and afforded 26.8 grs. of arid vitriol of zinc, which, redissolved in water, shot wholly into clear colourless prisms of this salt.

d. 9.2 grs. of these crystals, ignited in a covered tobacco-pipe, lost 3.2 grs. = 0.3478; hence, these crystals consist of,

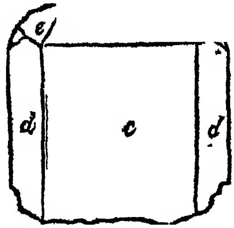
Carbonic acid - - -	0.348
Calx of zinc - - -	0.652
	1.000.

Electrical Calamine.

The Abbé HAUY has considered this kind as differing from the other calamines only in the circumstance of being in distinct crystals; but it has already appeared, in the instance of the Derbyshire calamine, that all crystals of calamine are not electric by heat, and hence, that it is not merely to being in this state that this species owes the above quality. And the following experiments, on some crystals of electric calamine from Regbania in Hungary, can leave no doubt of its being a combination of calx of zinc with quartz; since the quantity of quartz obtained, and the perfect regularity and transparency of these crystals, make it impossible to suppose it a foreign admixture in them.

a. 23.45 grs. of these Regbania crystals, displaced 6.8 grs. of distilled water, from a stopple-bottle, at the temperature of 64° FAHRENHEIT; their specific gravity is therefore = 3.434.

The form of these crystals is represented in the annexed Figure.



$ac = 90°$.
$ae = 150°$
$bc = 115°$
$cd = 130°$

They were not scratched by a pin; a knife marked them.

b. One of these crystals, exposed to the flame of the blow-pipe, decrepitated and became opaque, and shone with a green light, but seemed totally infusible.

Borax and microcosmic salt dissolved these crystals, without any effervescence, producing clear colourless glasses. Carbonate of soda had little if any action on them.

c. According to Mr. PELLETIER's experiments* on the calamine of Fribourg in Brisgaw, which is undoubtedly of this species, its composition is,

Quartz - - -	0.50
Calx of zinc - - -	0.38
Water - - - -	0.12
	1.00.

The experiments on the Regbania crystals have had different results; but, though made on much smaller quantities, they will perhaps not be found, on repetition, less in conformity with nature.

23.45 grs. heated red hot in a covered crucible, decrepitated a little, and became opaque, and lost 1.05 gr. but did not fall to powder or grow friable. It was found, that this matter was not in the least deprived of its electrical quality by being ignited; and hence, while hot, the fragments of these decrepitated crystals clung together, and to the crucible.

d. 22.2 grs. of these decrepitated crystals, = 23.24 grs. of the original crystals, in a state of impalpable powder, being digested over a spirit-lamp with diluted vitriolic acid, showed no effervescence; and, after some time, the mixture became a jelly. Exhaled to dryness, and ignited slightly, to expel the superfluous vitriolic acid, the mass weighed 37.5 grs.

* *Journal de Physique*, Tome XX. p. 424.

On extraction of the saline part by distilled water, a fine powder remained, which, after ignition, weighed 5.8 grs. and was quartz.

The saline solution afforded, on crystallization, only vitriol of zinc.

These crystals therefore consist of,

Quartz - - - -	0.250
Calx of zinc - - -	0.683
Water - - - -	0.044
	0.977
Loss - - -	0.023
	1.000.

The water is most probably not an essential element of this calamine, or in it in the state of, what is improperly called, water of crystallization, but rather exists in the crystals in fluid drops interposed between their plates, as it often is in crystals of nitre, of quartz, &c. Its small quantity, and the crystals not falling to powder on its expulsion, but retaining almost perfectly their original solidity, and spathose appearance in the places of fracture, and, above all, preserving their electrical quality wholly unimpaired, which would hardly be the case after the loss of a real element of their constitution, seem to warrant this opinion.

If the water is only accidental in this calamine, its composition, from the above experiments, will be,

Quartz - - -	0.261
Calx of zinc - - -	0.739
	1.000.

I have found this species of calamine amongst the productions of Derbyshire, in small brown crystals, deposited, together with

the foregoing small crystals of carbonate of zinc, on crystals of carbonate of lime. Their form seems, as far as their minuteness and compression together would allow of judging, nearly or quite the same as that of those from Regbania; and the least atom of them immediately evinces its nature, on being heated, by the strong electricity it acquires. On their solution in acids, they leave quartz.

OBSERVATIONS.

Chemistry is yet so new a science, what we know of it bears so small a proportion to what we are ignorant of, our knowledge in every department of it is so incomplete, so broken, consisting so entirely of isolated points thinly scattered like lucid specks on a vast field of darkness, that no researches can be undertaken without producing some facts, leading to some consequences, which extend beyond the boundaries of their immediate object.

1. The foregoing experiments throw light on the proportions in which its elements exist in vitriol of zinc. 23.0 grs. of the Mendip Hill calamine, produced 29.8 grs. of arid vitriol of zinc. These 23.0 grs. of calamine contained 14.9 grs. of calx of zinc; hence, this metallic salt, in an arid state, consists of *exactly equal* parts of calx of zinc and vitriolic acid.

This inference is corroborated by the results of the other experiments: 68.0 grs. of the Bleyberg calamine, containing 48.6 grs. of calx of zinc, yielded 96.7 grs. of arid vitriol of zinc; and, in another trial, 20.0 grs. of this ore, containing 14.2 grs. of calx of zinc, produced 28.7 grs. of arid vitriol of zinc. The mean of these two cases, is 62.7 grs. of arid vitriol of zinc, from 31.4 grs. of calx of zinc.

In the experiment with the crystals of carbonate of zinc from Derbyshire, 14.35 grs. of calx of zinc furnished indeed only 26.8 grs. of arid vitriol of zinc; a deficiency of about $\frac{6}{100}$, occasioned probably by some small inaccuracy of manipulation.

2. When the simplicity found in all those parts of nature which are sufficiently known to discover it is considered, it appears improbable that the proximate constituent parts of bodies should be united in them, in the very remote relations to each other in which analyses generally indicate them; and, an attention to the subject has led me to the opinion that such is in fact not the case, but that, on the contrary, they are universally, as appears here with respect to arid vitriol of zinc, fractions of the compound of very low denominators. Possibly in few cases exceeding five.

The success which has appeared to attend some attempts to apply this theory, and amongst others, to the compositions of some of the substances above analysed, and especially to the calamine from Bleyberg, induces me to venture to dwell here a little on this subject, and state the composition of this calamine which results from the system, as, besides contributing perhaps to throw some light on the true nature of this ore, it may be the means likewise of presenting the theory under circumstances of agreement with experiment, which, from the surprising degree of nearness, and the trying complexity of the case, may seem to entitle it to some attention.

From this calamine, containing, according to the results of the experiments on the Mendip Hill kind, too small a quantity of carbonic acid to saturate the whole of the calx of zinc in it, and from its containing much too large a portion of water to be in it in the state of mere moisture or dampness, it seems to

consist of two matters; carbonate of zinc, and a peculiar compound of zinc and water, which may be named *hydrate of zinc.*

By the results of the analysis of the Mendip Hill calamine, corrected by the theory, carbonate of zinc appears to consist of,

Carbonic acid - - -	$\frac{1}{3}$
Calx of zinc - - -	$\frac{2}{3}$.

Deducting from the calx of zinc in the Bleyberg calamine, that portion which corresponds, on these principles, to its yield of carbonic acid, the remaining quantity of calx of zinc and water is in such proportions as to lead, from the theory, to consider hydrate of zinc as composed of

Calx of zinc - - -	$\frac{3}{4}$
Water, or rather ice - - -	$\frac{1}{4}$.

And, from these results, corrected by the theory, I consider Bleyberg calamine as consisting of,

Carbonate of zinc - - -	$\frac{2}{5}$
Hydrate of zinc - - -	$\frac{3}{5}$.

The test of this hypothesis, is in the quantities of the remote elements which analysis would obtain from a calamine thus composed.

The following table will show how very insignificantly the calamine compounded by the theory, would differ in this respect from the calamine of nature.

1000 parts of the compound salt of carbonate and hydrate of zinc consist of,

Carbonate of zinc 400 =	Carbonic acid	$= \frac{400}{3}$	= - - -	$133\frac{1}{3}$
	Calx of zinc	$= \frac{400 \times 2}{3} = 266\frac{2}{3}$	} = -	$716\frac{2}{3}$
Hydrate of zinc = 600	Calx of zinc	$= \frac{600 \times 3}{4} = 450$		
	Ice - -	$= \frac{600}{4}$	= - -	150
				1000.

Great as is the agreement between the quantities of the last column and those obtained by the analysis of the Bleyberg calamine, (page 15,) it would be yet more perfect, probably, had there been, in this instance, no sources of fallacy but those attached to chemical operations, such as errors of weighing, waste, &c. but the differences which exist are owing, in some measure at least, to the admixture of carbonate of lime and carbonate of lead, in the calamine analysed, and also to some portion of water, which is undoubtedly contained, in the state of moisture, in so porous and bibulous a body.

It has also appeared, in the experiments on the Mendip Hill calamine, that acids indicate a greater quantity of carbonic acid than fire does, by $\frac{22}{1000}$. If we make this deduction for dissolved water, it reduces the quantity of carbonic acid in the Bleyberg calamine, to 0.1321.

If we assume this quantity of carbonic acid as the datum to calculate, on this system, the composition of the calamine from Bleyberg, we shall obtain the following results:

Compound salt, of carbonate of zinc and hydrate of zinc	990.3
Water, in the state of moisture - -	2.5
Carbonate of zinc and carbonate of lead - -	7.2
	1000.0

It may be thought some corroboration of the system here offered, that, if we admit the proportions which it indicates, the remote elements of this ore, while they are regular parts of their immediate products, by whose subsequent union this ore is engendered, are also regular fractions of the ore itself: thus,

The carbonic acid - - -	$=\frac{8}{60}$
The water - - -	$=\frac{9}{60}$
The calx of zinc - - -	$=\frac{43}{60}$

Hereby displaying that sort of regularity, in every point of view of the object, which so wonderfully characterises the works of nature, when beheld in their true light.

If this calamine does consist of carbonate of zinc and hydrate of zinc, in the regular proportions above supposed, little doubt can exist of its being a true chemical combination of these two matters, and not merely a mechanical mixture of them in a pulverulent state; and, if so, we may indulge the hope of some day meeting with this ore in regular crystals.

If the theory here advanced has any foundation in truth, the discovery will introduce a degree of rigorous accuracy and certainty into chemistry, of which this science was thought to be ever incapable, by enabling the chemist, like the geometrician, to rectify by calculation the unavoidable errors of his manual operations, and by authorising him to discriminate from the essential elements of a compound, those products of its analysis whose quantity cannot be reduced to any admissible proportion.

A certain knowledge of the exact proportions of the constituent

principles of bodies, may likewise open to our view harmonious analogies between the constitutions of related objects, general laws, &c. which at present totally escape us. In short, if it is founded in truth, its enabling the application of mathematics to chemistry, cannot but be productive of material results.*

3. By the application of the foregoing theory to the experiments on the electrical calamine, its elements will appear to be,

Quartz - - -	$\frac{1}{4}$
Calx of zinc - - -	$\frac{3}{4}$

A small quantity of the calamine having escaped the action of the vitriolic acid, and remained undecomposed, will account for the slight excess in the weight of the quartz.

4. The exhalation of these calamines at the blowpipe, and the flowers which they diffuse round them on the coal, are probably not to be attributed to a direct volatilization of them. It is more probable that they are the consequences of the dis-oxidation of the zinc calx, by the coal and the inflammable matter of the flame, its sublimation in a metallic state, and instantaneous recalcination. And this alternate reduction and combustion, may explain the peculiar phosphoric appearance exhibited by calces of zinc at the blowpipe.

The apparent sublimation of the common flowers of zinc at the instant of their production, though totally unsublimable afterwards, is certainly likewise but a deceptious appearance. The reguline zinc, vaporized by the heat, rises from the crucible as a metallic gas, and is, while in this state, converted to a calx. The flame which attends the process is a proof of it; for flame is a mass of vapour, ignited by the production of fire within itself.

* It may be proper to say, that the experiments have been stated *precisely* as they turned out, and have not been in the *least degree* bent to the system.

The fibrous form of the flowers of zinc, is owing to a crystallization of the calx while in *mechanical suspension* in the air, like that which takes place with camphor, when, after having been some time inflamed, it is blown out.

A moment's reflection must evince, how injudicious is the common opinion, of crystallization requiring a state of solution in the matter; since it must be evident, that while solution subsists, as long as a quantity of fluid admitting of it is present, no crystallization can take place. The only requisite for this operation, is a freedom of motion in the masses which tend to unite, which allows them to yield to the impulse which propels them together, and to obey that sort of polarity which occasions them to present to each other the parts adapted to mutual union. No state so completely affords these conditions as that of mechanical suspension in a fluid whose density is so great, relatively to their size, as to oppose such resistance to their descent in it as to occasion their mutual attraction to become a power superior to their force of gravitation. It is in these circumstances that the atoms of matters find themselves, when, on the separation from them of the portion of fluid by which they were dissolved, they are abandoned in a disengaged state in the bosom of a solution; and hence it is in saturated solutions sustaining evaporation, or equivalent cooling, and free from any perturbing motion, that regular crystallization is usually effected.

But those who are familiar with chemical operations, know the sort of agglutination which happens between the particles of subsided very fine precipitates; occasioning them, on a second diffusion through the fluid, to settle again much more quickly than before, and which is certainly a crystallization, but under circumstances very unfavourable to its perfect performance.

5. No calamine has yet occurred to me which was a real, uncombined, calx of zinc. If such, as a native product, should ever be met with in any of the still unexplored parts of the earth, or exist amongst the unscrutinized possessions of any cabinet, it will easily be known, by producing a quantity of arid vitriol of zinc exactly double its own weight; while the hydrate of zinc, should it be found single, or uncombined with the carbonate, will yield, it is evident, 1.5 its weight of this arid salt.

III. *Experiments on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures.* By Mr.* William Henry. *Communicated by the Right Hon. Sir* Joseph Banks, *K. B. P. R. S.*

Read December 23, 1802.

Though the solubility of an individual gas in water forms, generally, a part of its chemical history, yet this property has been overlooked, in the examination of several species of the class of aëriform substances. The carbonic acid, indeed, is the only gas whose relation to water has been an object of much attention; and, at a very early period of its history, Mr. Cavendish, in the course of inquiries, the results of which were the groundwork of the most important subsequent discoveries, ascertained, with peculiar care, the proportion of carbonic acid gas condensible in water, at the temperature of 55° of Fahrenheit. Dr. Priestley also, about the same period, directed his attention to the saturation of water with fixed air, and contrived a simple and effectual mode of obtaining this impregnation. His apparatus, afterwards, gave way to the more manageable one of Dr. Nooth; and this, in its turn, has been superseded by the improved mode of condensing, into water, many times its bulk of various gases, invented and practised by several chemical artists, (as well as by myself,) both in this country and abroad.

The influence of pressure, in accomplishing this strong impregnation, was first, I believe, suggested by Dr. Priestley.

" In an exhausted receiver," that most ingenious philosopher observes, " Pyrmont water will actually boil, by the copious " discharge of its air; and I do not doubt, therefore, that by " means of a condensing engine, water might be much more " highly impregnated with the virtues of the Pyrmont " spring." *

Before describing my experiments on the effects of additional pressure, in saturating water with gases, it will be necessary to state the results of others, that were previously expedient, to determine the quantity of each gas combinable with water, at a given temperature, and under the ordinary weight of the atmosphere. In a few instances, also, it was deemed proper to ascertain the influence of different temperatures, over the condensation of gases in water.

SECTION I.

ON THE QUANTITY OF GASES ABSORBED BY WATER, UNDER THE USUAL PRESSURE OF THE ATMOSPHERE.

In order to attain considerable minuteness in observing the proportion of gases absorbed by water, an apparatus was employed, of which the following is a description.

The vessel A (Plate I. Fig. 1) is of glass, about 2 inches diameter, and 14 inches long. It is graduated into cubical inches, and quarter inches; and furnished at the top with a brass cap, into which a cock *a* is screwed. To the lower aperture, a copper tube C is cemented, which is bent at a right angle, the leg nearest the vessel being carried downwards, and furnished with a cock *b*. B is a glass tube, of about $\frac{1}{4}$ inch bore,

* Experiments on Air, arranged and methodized, Vol. I. p. 51.

bent at a right angle, and graduated, from a given point, into hundredth parts of a cubical inch. It is attached to the copper pipe, by a tube of Indian rubber D, over which is a covering of leather, forming a joint, which admits of the vessel A being briskly agitated. When the apparatus is used, it is first filled with quicksilver; a transfer bottle of elastic gum, furnished with a cock, and containing water of a known temperature, is screwed on; and a communication is opened, through the cocks, between the bottle and the glass vessel. The lower cock *b* is then opened, through which the mercury runs out, while its place is supplied by a quantity of water from above, measurable by the scale on A. This transfer is removed, and another containing gas being substituted, a measured quantity of gas is admitted in a similar manner. Strong agitation is now applied, by means of the joint D; and mercury is poured into the tube B, to supply the descent occasioned by the absorption in A; its level being exactly preserved in both legs of the syphon, both at the commencement and close of the experiment. The quantity of mercury required for this purpose, indicates precisely the amount of the gas absorbed.

The only advantage of this apparatus over a cylindrical jar, inverted in the usual way over mercury, is, that by means of the tube B, very minute degrees of absorption may be measured, which would scarcely be perceived in a wide vessel.

For the more absorbable gases, I found this instrument to answer perfectly well; but, for ascertaining the solubility of those which are taken up by water in only small proportion, I preferred one of different construction. It consisted simply of a glass vessel, of the capacity of $57\frac{1}{2}$ cubical inches, and shaped as in Fig. 2. At *a* was cemented a cock, provided with a screw;

and the lower cock *b* was of glass, accurately ground in. The vessel was then filled with water which had been long boiled; a lifting valve was screwed on *a*, the cock being open; and the vessel was placed under the receiver of an air pump, where it was kept for some time, the pump being occasionally worked, as long as any air bubbles could be seen to arise. The gas under examination was next admitted from an elastic bottle, the cock *b* being opened, and a measured quantity of water let out. The gas and water were then violently agitated together; and the cock *b* opened under mercury, which ascended into the vessel. The agitation was still continued, observing to preserve the same level of mercury without as within the vessel; and, when it rose no higher, the ascent was noted by means of the graduated scale. The quantity of mercury that had entered the vessel, indicated the amount of absorption that had ensued.

It might, however, be objected, that the water would acquire air again, while poured into the vessel; and I therefore sometimes used large glass globes, having long necks, accurately graduated. These globes, being of very thin glass, were filled with boiling water, and inverted instantly in a trough of quicksilver. When the water became cold, the mercury was, of course, found to have risen partly into the vessel. This portion was displaced by a measured quantity of gas; and the absorption was denoted by the ascent of the mercury in the graduated neck.

The water employed in these experiments was boiled, during several hours, in a tin vessel having an aperture barely sufficient to allow the egress of the steam, and poured, while boiling hot, into glass vessels, which were corked, and tightly tied over with bladder. An equable temperature was produced in the

water, mercury, and gas, except when above 85°, by regulating that of the room in which the experiments were made; and the glass vessel, during agitation, was carefully guarded from the warmth of the hand. The agitation was continued, till it appeared, by the scale, to produce no further effect; and, in the absorption of difficultly condensible gases, was repeated at intervals, during a space of from twelve to twenty-four hours. Alterations of the barometer were always observed; and the residuary gas measured, or estimated, at a pressure of $29\frac{1}{2}$ inches.

1. *Absorption of Carbonic Acid Gas by Water.*

That the temperature of water influences the proportion of carbonic acid which it is capable of absorbing, is already known as a general fact;* but the exact amount of this influence has not, I believe, been hitherto ascertained. In the course of a series of experiments to determine it with precision, I was surprised by obtaining results which differed considerably from each other, at the same temperature of the gas and water; when both were, in different experiments, of like purity; and when the barometer had the same elevation. Of the cause of these variations I was not aware, till my friend Mr. DALTON suggested, that they probably depended on the variable amount of the residues; and, on repeating the experiments, with different proportions between the gas and the water, this suggestion was fully confirmed. Thus, when two measures of carbonic acid gas were agitated with one measure of water, the absorption was considerably greater than when, to the same quantity of water, a less proportion of gas was used. The cause of this diminished

* See Mr. CAVENDISH's experiments in the Phil. Trans. Vol. LVI. p. 163; and FOURCROY's *Système*, 4to. Tom I. p. 215.

absorption, seems to be connected with the proportion of common air contained in the unabsorbed residuum; for, besides the unavoidable contamination of the gas employed, with a minute portion of the air of the vessel used for its extrication, a small quantity will always be liberated from the water, whatever pains have been taken to deprive it of air, by previous long boiling, exposure under the air pump, or both in succession. That this is the true explanation, appears also, from the result of adding to the gas a proportion of common air. Thus, when, at the temperature of 55°, 20 measures of carbonic acid are agitated with 10 of water, at least 10 measures of gas are taken up; but, from a mixture of 20 measures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid, or 4 less than in the former instance.

An analogous fact was observed by Dr. BROWNRIGG,* who remarked that gas does not escape from the water which it impregnates, unless the water be in contact with air: for, when the Pouhon water was excluded from air, but, at the same time, liberty was given for its gas to arise into an empty bladder, the gas did not spontaneously separate from the water; but, on the contrary, remained united with it, when exposed to the greatest heat of our climate. When the impregnated water, he observes, is thus excluded from air, the gas will escape very slowly, at any temperature less than 110° of FAHRENHEIT, although such heat be sufficient for the distillation of water; nor can it be wholly expelled by a heat of 160° or 170°, continued two hours. But it is well known, that water saturated with carbonic acid gives up its gas rapidly, when freely exposed to the atmosphere.

* See Dr. BROWNRIGG's Paper on the Pouhon Water, Phil. Trans. Vol. LXIV.

In fixing the proportion of carbonic acid absorbed, it is therefore necessary to note the quantity of residuum, as is done in the following table.

Experiment.	Temperature.	Measures of water.	Measures of gas.	Quantity absorbed.	Residue.	Absorbed by 100 inches of water.
1	55	13	32	14	18	108
2	85	13	32	11	21	84
3	55	13	24	14	10	108
4	55	10	15	10	5	100
5	55	20	20	18	2	90
6	55	19	19	16	3	84
7	85	19	19	13	6	70
8	110	10	20	6	14	60
9	110	20	20	9	11	45

Since the above table was drawn up, I have been gratified by remarking that, in the experiments of Mr. CAVENDISH, similar variations in the quantities absorbed, were produced by the variable amount of the residua; as will appear from the following deductions from his experiments.

At the temperature of 55°.

1. When the gas absorbed was to the residue as 100 to 164, 100 cubical inches of water took up - - 116
2. When the absorbed gas was to the residue as 100 to 16, 100 inches of water took up - - - 107
3. The absorbed gas being to the residue as 100 to 10, 100 parts of water absorbed - - - 102½
4. The absorption being to the residue as 100 to 1½, 100 parts of water took up - - - 95½

The quality of the residuum, I only ascertained in experiments 5 and 6 of the preceding table. In experiment 5, the residuary two measures contained 7½ per cent. of common air,

or 0.15 of a measure. But, of those, .13 existed previously in the 20 measures of carbonic acid gas; and the 20 measures of water had, therefore, only given up .02 of a measure, or about $\frac{1}{1000}$ of its bulk. I apprehend, however, that the whole of the common air was not, even thus, extricated from the water. In experiment 6, the 3 residuary measures contained $\frac{1}{6}$ of common air.

To judge of the influence of temperature, it is essential that the experiments compared should be on similar proportions of gas and water. Thus, from a comparison of experiment 1 and 2, it appears, that about $\frac{1}{14}$ of the whole bulk absorbable at 55°, is the diminution of the quantity of absorption produced by each elevation of 10° of temperature; and the same inference follows from various other experiments, the results of which I have thought it needless to state.*

2. *Sulphuretted Hydrogen Gas.*

One hundred parts of water, at 55° of temperature, absorb 86 parts of this gas, obtained from sulphuret of iron and dilute sulphuric acid, a residue being left, equal in bulk to the gas absorbed. At 85°, under similar circumstances, the same quantity absorbs 78.

3. *Nitrous Oxide.*

At 45°, 100 cubic inches of water take up 50 of nitrous oxide; and, at 70°, the same quantity takes up only 44. According to Mr. DAVY, in whose experiments, from his intimate

* During the absorption of carbonic acid by water, the gas and water having previously the same temperature, there is an extrication of caloric, sufficient to raise the temperature of the water between $\frac{1}{2}$ and $\frac{3}{4}$ of a degree of FAHRENHEIT. The same effect is produced by the condensation of sulphuretted hydrogen, and nitrous oxide gases, though less apparently. To perceive this phenomenon, considerable quantities of gas

knowledge of this gas, and skill in its preparation, I place more confidence than in my own, 100 inches of water at 45°, take up 54 of nitrous oxide, the residuum being about one half the volume of the gas absorbed.

4. *Less absorbable Gases.*

The experiments with those gases which are absorbed only in sparing proportion by water, I could not conveniently make at more than one temperature; nor, indeed, did the object appear to me worthy of the time and attention which such a repetition of them would have required. Of the accuracy of the following, however, I satisfied myself, by repeating each two or three times; and with gases of the greatest attainable purity.

100 cubic inches of water, at 60°, absorb,

Of nitrous gas - - -	5	inches.
Oxygenous gas - - -	2.63	
Phosphuretted hydrogen ditto -	2.14	
Gaseous oxide of carbon - -	2.01	
Carburetted hydrogen gas - -	1.40	
Azotic gas - - -	1.20	
Hydrogen gas - - -	1.08	

The solubility of atmospherical air cannot easily be ascertained; for, as I shall hereafter shew, in a memoir on the expulsion of gases from water by each other, air is decomposed by agitation with boiled water, its oxygenous portion being absorbed in preference.

From the statements given by various philosophers, (the Abbé Nollet, Drs. Hales, Priestley, and Pearson,) of the quantity of air separable from water of different kinds, by heat or a diminished pressure, I expected that a much larger proportion of the gases constituting the atmosphere would have been absorbed by water, than the above numbers assign. It is to be

recollected, however, that no method hitherto discovered detaches from water all its air; and the unknown quantity remaining in it, after these modes of separation have been employed, is to be added to that with which a given volume of water can be artificially impregnated. Dr. Pearson, in his enquiries into the nature of the gas obtained by passing electric discharges through water, was at great pains to purify the subject of his experiments from air, by boiling and a powerful air pump; but he always found, that after the full effect of both these methods, electricity liberated a further, and not an inconsiderable, portion of air.*

Common spring water may, I think, be fairly taken as a specimen of water fully charged with atmospherical air; and, with the view of determining the quantity and kind of gases extricated from it, I made the following experiment. A glass globe, of the capacity of 117½ cubical inches, was filled with water fresh from the well. To its mouth was adapted a curved and stoppered tube, which held ¾ of an inch; and this was also filled with water. The globe was then placed in a vessel of brine, which was kept boiling between six and seven hours; and the gases were received over mercury. Their quantity and quality were as follows.

No.	Cub. inches.	Consisted of		Proportion of Oxygen gas in the residuary air.
		Carbonic acid.	Air.	
1	1.25	0.50	0.75	0.20
2	1.25	0.85	0.40	0.16
3	1.63	1.23	0.40	0.16
4	0.50	0.49	0.01	lost by accident,
	4.63	3.07	1.56	
Air remaining in the bent tube	0.75			
	5.38, total gas from 117½ inches of water.			

* Phil. Trans. for 1797.

But, $4\frac{1}{2}$ inches of water were expelled, owing to the expansion by heat. Therefore, $117\frac{1}{2} - 4\frac{1}{2} = 113$ inches of water, gave 5.38 inches of gas; and 100 inches, consequently, gave 4.76, of which 3.38 were carbonic acid, and 1.38 atmospherical air. Hence, the water afforded about $\frac{1}{70}$ its bulk of atmospherical air, and $\frac{1}{20}$ of a mixture of gases. In this estimate, the gas remaining in the tube is reckoned as carbonic acid, which may be allowed, since the portion last obtained held only $\frac{1}{50}$ its bulk of common air.

SECTION II.

ON THE INFLUENCE OF PRESSURE IN PROMOTING THE ABSORPTION OF GASES; AND THE DESCRIPTION OF AN APPARATUS FOR EXHIBITING THIS PHENOMENON.

For the purpose of determining the ratio between the addition of pressure and the increased absorption of gases by water, I employed the apparatus, with some addition, which has been already described. The tube B was lengthened at pleasure, with the view of obtaining, by a column of mercury, any additional pressure that might be required. The vessel A, Fig. 1, was then filled completely with mercury, which rose to its corresponding level in the tube B. A given quantity of water, of a known temperature, and afterwards a measured volume of gas, were transferred into the vessel, in the mode already described; and, as the mercury, by opening the cock *b*, was brought to the same level in both legs of the syphon, the gas, it is evident, must have been under the ordinary weight of the atmosphere. A quantity of mercury was next poured into the leg B, sufficient to form a column 28 inches higher than the level of the mercury in A, after this addition; and the bulk of the gas was again noted. This was found to be, pretty exactly, $\frac{1}{2}$, $\frac{1}{3}$, &c. of

the space occupied before, when one, two, or more additional atmospheres were applied. Brisk agitation was now used, as long as any absorption took place; and, into the tube B an assistant poured mercury, so as to preserve in it the excess of 28 inches above the level of the mercury in A. The degree of absorption was known by the scale on A, or, more accurately, by the quantity of mercury required to support the elevation of 28 inches in B.

By lengthening the column in B to 56 inches, the pressure of two additional atmospheres was obtained; and this was the utmost extent to which the addition of weight could be carried, without forcing the joint at D.

When the cock *b* was opened, and the column in each leg thus suddenly fell to the same level, the water, which had been previously charged with gas, under a pressure of three atmospheres, effervesced violently; but some time elapsed before the additional gas, forced in by compression, was wholly evolved. These appearances are very striking and amusing; and are well calculated for exhibition in a chemical lecture. The apparatus, however, I have no doubt, may be greatly improved; but, at the distance of nearly 200 miles from the metropolis, I was under the necessity of using such an one as could be constructed by my own hands.

A considerable improvement in the construction of the apparatus, which would obviate the expediency of the flexible tube D, would be the following. To the lower neck of the vessel A, Fig. 1, let a cap and cock, with a female screw, be cemented; and let the upper end of the pipe C be terminated by a cock with a male screw. Introduce the gas and water, in the manner already described; apply the increased pressure; and, having shut the two additional cocks, unscrew them from each other.

The vessel A will thus be detached, and agitation may be easily applied; after which, again screw it into its former place, and, on opening the two cocks, the mercury will rise in the vessel A. Supply the descent in B by fresh mercury, and proceed as before, repeating alternately the pressure and agitation, as long as any further absorption takes place.

A further amendment of the apparatus, would consist in the substitution of cocks of some other metal than brass, which, however perfect at first, are always injured by the repeated action of the mercury. If cocks of glass could be ground sufficiently tight, metal caps with screws might be cemented to them.

For observing the increased absorption of less condensible gases, I found it necessary to substitute a vessel of larger size than A, and of the capacity of at least 50 cubical inches. It is represented by the dotted lines in Fig. 1, and was furnished with a cock and screw at *c*. As it would have been troublesome to have filled so large a vessel entirely with quicksilver, it was filled with boiled water, with the exception of a quantity of quicksilver rather exceeding the bulk of the gas employed. The gas was admitted, as usual, from a transfer bottle, the mercury which it replaced escaping through the cock *b*. The increased pressure was next applied; and the experiment conducted as before, except that the agitation was much longer continued.

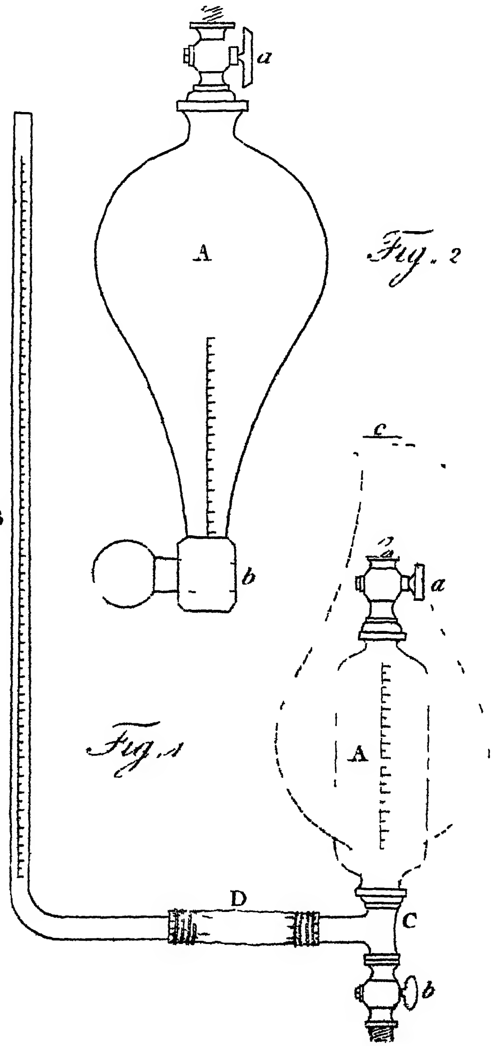
The results of a series of at least fifty experiments, on carbonic acid, sulphuretted hydrogen gas, nitrous oxide, oxygenous and azotic gases, with the above apparatus, establish the following general law: *that, under equal circumstances of temperature, water takes up, in all cases, the same volume of condensed gas as of gas under ordinary pressure.* But, as the spaces occupied

by every gas are inversely as the compressing force, it follows, *that water takes up, of gas condensed by one, two, or more additional atmospheres, a quantity which, ordinarily compressed, would be equal to twice, thrice, &c. the volume absorbed under the common pressure of the atmosphere.* By frequent repetition of the experiments, I obtained results differing a little from the general principle above stated; but, for all practical purposes, I apprehend, the law has been announced with sufficient accuracy.*

In place of the cock *a*, I cemented, in one experiment, a very sensible thermometer. The vessel was next filled with mercury through the cock *b*; and the tube B being also filled, the cock *b* was shut, and a bottle of carbonic acid gas screwed on. The cock *b* being then opened, the mercury descended, and a measured quantity of carbonic acid arose into the vessel A. In the same way, a measured quantity of water was introduced. When the density of the air was suddenly doubled by a column of quicksilver, the mercury in the thermometer, whose bulb was still surrounded by the condensed gas, rose about $1\frac{1}{2}$ degree. On agitating the vessel, till the water encompassed the bulb of the thermometer, an elevation of barely $\frac{1}{2}$ a degree ensued in the temperature of the water. This ascent would probably have been greater, if the evolved heat had not been carried off by the mercury on which the water floated.

Manchester,
Dec. 8th, 1802.

* That the facts did not, with invariable accuracy, correspond to the law, was perhaps, in part, owing to the addition of only 28 inches of pressure; when, in strictness, $29\frac{1}{2}$ should have been used, or twice the elevation of the mercury in the barometer, during each experiment.


a
A
Fig. 2
b
B
c
a
A
Fig. 1
D
C
b

IV. *Experiments and Observations on the various Alloys, on the specific Gravity, and on the comparative Wear of Gold. Being the Substance of a Report made to the Right Honourable the Lords of the Committee of Privy Council, appointed to take into Consideration the State of the Coins of this Kingdom, and the present Establishment and Constitution of his Majesty's Mint. By* Charles Hatchett, *Esq. F. R. S.*

Read January 13, 1803.

LIST *of the* COMMITTEE *appointed on the* 10*th of February,* 1798, *to take into Consideration the State of the Coins of this Kingdom.*

EARL OF LIVERPOOL, PRESIDENT.
THE LORD HIGH CHANCELLOR OF GREAT BRITAIN.
THE LORD PRESIDENT OF THE COUNCIL.
THE LORD PRIVY SEAL.
HIS MAJESTY'S PRINCIPAL SECRETARIES OF STATE.
THE MASTER GENERAL OF THE ORDNANCE.
THE FIRST LORD COMMISSIONER OF THE ADMIRALTY.
THE FIRST LORD COMMISSIONER OF HIS MAJESTY'S TREASURY, AND CHANCELLOR OF THE EXCHEQUER.
HIS MAJESTY'S SECRETARY AT WAR.
HIS GRACE THE DUKE OF MONTROSE.
THE LORD CHIEF JUSTICE OF HIS MAJESTY'S COURT OF KING'S BENCH.

THE SPEAKER OF THE HOUSE OF COMMONS.
THE MASTER OF THE ROLLS.
THE LORD CHIEF JUSTICE OF HIS MAJESTY'S COURT OF COMMON PLEAS.
THE LORD CHIEF BARON OF HIS MAJESTY'S COURT OF EXCHEQUER.
THE VICE PRESIDENT OF THE COMMITTEE OF COUNCIL FOR TRADE.
THE RIGHT HON. SIR JOSEPH BANKS, BART. K. B.
THE RIGHT HON. SIR WILLIAM WYNNE.
THE RIGHT HON. SYLVESTER DOUGLAS.

INTRODUCTION.

THE Lords of the Committee of his Majesty's most honourable Privy Council, appointed by his Majesty, on the 10th of February, 1798, to take into consideration the state of the coins of this kingdom, having among other circumstances remarked the considerable loss which the gold coin appeared to have sustained by wear within certain periods, and being desirous to ascertain whether this loss was occasioned by any defect, either in the quality of the standard gold or in the figure or impression of the coins, were pleased to request that HENRY CAVENDISH, Esq. F. R. S. and myself would examine, by such experiments as should be deemed requisite, whether any of these defects really existed.

Two questions were to be principally decided,

1st. Whether very soft and ductile gold, or gold made as hard as is compatible with the process of coining, suffers the

most by wear, under the various circumstances of friction to which coin is subjected in the course of circulation?

2dly. Whether coin with a flat, smooth, and broad surface, wears less than coin which has certain protuberant parts raised above the ground or general level of the pieces?

Concerning the first question, opinions were various, and the most intelligent persons were uncertain whether very soft or hard gold was to be preferred; and, in respect to the second question, it must be observed, that although the prevalent opinion was in favour of flat and smooth surfaces, yet, as the fact had never been fully and satisfactorily determined, this opportunity was embraced, in order that every doubt might be removed.

The great value of the material, had hitherto prevented private individuals from ascertaining these facts by experiment; and, as a public concern, this subject of investigation, although so important to political economy and to science, does not appear to have been noticed by any European government, until the Right Honourable and enlightened Members of the abovementioned Committee proposed the inquiry, and furnished the requisite means for making the experiments.*

At the request of Mr. Cavendish, I have written the following account; but I should be highly unjust and ungrateful to that gentleman, did I not here publicly acknowledge how great a portion truly belongs to him, of any merit which these experiments may be found to possess: for, at all times, I was favoured with his valuable advice; and the machines to produce friction, as well as the dies, were entirely contrived by himself. At the

* These experiments were begun in the latter end of 1798, and were completed in April, 1801.

same time, I wish it to be understood, that I alone am to be considered as responsible for any inaccuracies of the experiments.

Lastly, before I proceed, I must take this opportunity to acknowledge my obligations to JAMES MORRISON, Esq. the Deputy Master of the Mint, to Mr. JAMES MORRISON, his son, to ROBERT BINGLEY, Esq. his Majesty's Assay Master, and to Messrs. ATKINSON and NICHOLL, of the Corporation of Moneyers, for the ready assistance and polite attention which I received from those gentlemen, during the long series of experiments made at the Mint.

SECTION I.

ON THE VARIOUS ALLOYS OF GOLD.

The wear of coin is an effect produced by mechanical causes, subject to be modified by certain physical properties, such as ductility and hardness, which vary in degree, according to the chemical effects produced by different metallic substances, when employed in certain proportions as alloys. From these considerations, it appears proper,

First, to examine the effects which the various metals produce upon gold, when combined with it in given proportions, beginning with $\frac{1}{12}$, which is the standard proportion of alloy, and in certain cases gradually decreasing to $\frac{1}{4}$ of a grain in the ounce Troy, or $\frac{1}{1920}$ part of the mass.

Secondly, to examine the specific gravity of gold differently alloyed, and the causes of certain variations to which it is liable.

And, thirdly, to ascertain the effects of friction variously modified.

GOLD ALLOYED WITH ARSENIC.

Experiment I.

Eleven ounces one pennyweight and three grains (=5307 grs.) of gold, 23 carats $3\frac{1}{2}$grs. fine, being completely melted, eighteen pennyweights and twenty-one grains (=453 grs.) of pure metallic arsenic were added, and the whole being rapidly stirred, was quickly poured into a greased mould of iron.

The bar was of the colour of fine gold, and, although brittle, yet it bent in some measure before it broke. It weighed eleven ounces one pennyweight and nine grains; so that, of 18 dts. 21 grs. of arsenic, only six grains remained in combination with the gold; consequently, 18 dts. 15 grs. had been volatilized.

Experiment II.

As the fine gold, in the foregoing experiment, retained so very small a portion of the arsenic, it appeared possible that copper might assist to fix that volatile substance.

To eighteen pennyweights and ten grains of the fine gold in fusion, nineteen grains of pure copper were added, being half the weight of the standard proportion of alloy.

When the copper was perfectly melted, and, by stirring, had been well incorporated with the gold, the crucible was removed, and at that moment nineteen grains of arsenic were added, and being quickly stirred, the metal was immediately poured into a mould.

The time which elapsed from the raising of the crucible to the pouring of the metal, was rather less than one minute; but, upon weighing the ingot, it appeared that the whole of the 19 grains of arsenic had been volatilized; and this was

corroborated by the perfect ductility which the gold was found to possess.

In this experiment, the whole of the arsenic was separated; and we may conclude, that it is always difficult to combine arsenic with gold by mere addition in open vessels, and that when to a small quantity of gold in fusion, a small quantity of arsenic is added, it is immediately dissipated by the violence of the heat; but, if large quantities are employed, and the metal is poured as soon as possible after the addition of the arsenic, then, according to circumstances, a small portion may remain combined with the gold.

It is well known that arsenic may be easily combined with gold and other metals, when in fusion, by employing a mixture of oxide of arsenic and black flux, and performing the operation in close vessels; but the following experiment will prove, that arsenic may at all times be combined with gold, provided the latter, when it loses its heat and congeals, is surrounded by arsenical vapour.

Experiment III.

480 grains of fine gold were put into a four-inch crucible, which was then placed within a large one that measured about 12 inches. At the bottom of this last, and on the outside of the small crucible, one ounce of metallic arsenic was placed, and another large crucible was then closely luted, with its mouth inverted upon that of the lower one.

The whole was exposed to a strong heat in a wind-furnace, during two hours, after which, the vessels were suffered gradually to become cold. Upon removing the upper crucible, which formed the dome, some white oxide of arsenic was found adhering to the inverted bottom of it.

2dly. The remainder of the metallic arsenic coated the bottom of the inferior large crucible.

3dly. On the sides of the upper crucible or dome were several small globules of gold. And,

4thly. The button of gold in the small crucible, although unchanged in external appearance, was found to be extremely brittle, and, when broken, the fracture appeared of a coarse grain, and of a gray colour.

The button weighed 481,5 grs. so that, exclusive of the gold which had been volatilized, there was an increase of the original weight, amounting to 1,5 gr.

When arsenic is by any means combined with gold, it is not easy to separate it totally by mere heat; for, although this button was twice kept in strong fusion, during one hour each time, in an open crucible, it still retained some arsenic, and continued to be brittle.

From this last experiment it is evident, that a considerable degree of affinity prevails between gold and arsenic; but, as the latter is immediately volatilized at the instant of contact with melted gold, it cannot easily be combined with it when open vessels are employed, and when the arsenic is simply added to the gold in fusion, while so great a degree of heat is continued.

This volatility of arsenic is, on the contrary, in favour of the combination when the operation is performed in close vessels; for, as arsenic is reduced to a state of vapour by heat much inferior to that which is requisite to the fusion of gold, and as this vapour remains included during the melting and cooling of the gold, it necessarily follows, that the gold is cooled, and becomes solid, while immersed in the arsenical atmosphere, so that the state of the gold, the extreme division of the arsenic,

and the gradual cooling of the vessels, in every way promote the union of the two metals.

ADDITIONAL EXPERIMENTS UPON GOLD AND ARSENIC.

Since the above experiments were written, I was induced to examine what effects would be produced by arsenic, in the state of vapour, upon red-hot plates of standard gold, the alloy of which was copper.

With the assistance of Mr. BINGLEY, I therefore made the following experiments.

I.

Two six-inch crucibles were ground so as to fit close when the mouth of one was inverted upon the other. Within the upper crucible or dome, a plate of the standard gold, which was $2\frac{1}{2}$ inches long, $1\frac{1}{2}$ broad, and $\frac{1}{30}$ thick, and slightly bent, was suspended by a strong iron wire; and one ounce of metallic arsenic was put into the lower crucible. The vessels were then closely luted, and were placed in an open fire, so that they became of a full red heat; they were kept in this state about 12 or 15 minutes, after which, they were removed from the fire, and when cold were opened.

I then found, that although the heat had been so very much inferior to that which is requisite to cause the fusion of gold, yet, in the present case, some very extraordinary effects had been produced; for the arsenic, which had been resolved into vapour, had acted upon the red hot plate of gold, and had combined with every part of its surface; but the combination so formed, being extremely fusible, had immediately separated from the remaining plate of metal, and had fallen into the lower crucible, where it had formed an ingot or button. This ingot

was internally of a gray colour,* and was extremely brittle; but the plate from which the portion of gold had been separated, remained perfectly ductile, and did not retain any of the arsenic; for, although it was superficially discoloured, it was unchanged in every other respect, excepting that the sharpness of its edges was destroyed, and the thickness was reduced from $\frac{1}{30}$ of an inch to that of common writing paper.

The effects produced by the arsenic, in this experiment, were very remarkable; for the plate was as uniformly and evenly reduced in its thickness as if it had been planed; and, while the portion which had dropped from it was combined with a very large quantity of arsenic, the remaining part of the plate appeared to have preserved the whole of its original ductility and purity.

These singular effects took place, as I have already observed, in so short a time as 12 or 15 minutes; but I wished to ascertain whether the same would not happen, more or less, within a smaller period, and under circumstances not so favourable to the union of arsenic with gold.

II.

Two six-inch crucibles were fitted, and inverted in the manner already described, and a plate of the standard gold, similar in size and quality to that which had been employed in the first experiment, was in like manner suspended within the upper crucible or dome.

A semicircular piece was cut out of the lip of this crucible, so that, when inverted and luted upon the lower vessel, there was an aperture of about $1\frac{1}{2}$ inch in diameter, which was

* The external colour was like that of fine gold, in consequence of the arsenic having been volatilized from the surface of the ingot, by the heat of the lower crucible.

loosely stopped with a piece of charcoal, and the crucibles, being firmly luted, were as before placed in an open fire.

When the vessels appeared of a full red heat, they were taken out, and, being placed upon the pavement of the laboratory, about half an ounce of metallic arsenic was quickly introduced through the aperture that has been mentioned, which was again closed, although very imperfectly, by a piece of charcoal. The arsenic immediately began to produce flame and fumes, which partially escaped through the opening; in about five minutes, the crucibles ceased to appear red-hot, and the greater part of the arsenic was dissipated.

Upon separating the crucibles, the plate of gold was found entire, but it was much discoloured; and the portion of gold which had combined with the arsenic, had trickled to the edges of the plate, where it became accumulated, and would soon have dropped into the lower crucible, had it not been for the short duration of the heat.

The plates of gold which were employed in the two experiments, had been annealed, and were remarkably ductile; and it has already been observed, that the part of the plate which remained after the first experiment, completely retained its original ductility; but the plate which had been employed in the second experiment, although not brittle, was become less flexible. The cause of this difference was very apparent; for, in the first experiment, the whole of the gold combined with arsenic had, by the continuance of the heat, been enabled to flow from the remaining part of the plate of standard gold, which, although thus reduced in size, retained none of the arsenic.

In the second experiment, on the contrary, the heat ceased,

almost as soon as a portion of the arsenic had united with the gold; and the whole surface of the plate, therefore, remained thinly coated with arsenicated gold, by which, a certain degree of rigidity was produced.

These experiments prove, that arsenic, in the state of vapour, will readily combine with gold, even when the latter is only raised to a common red heat. But the whole substance of the gold is not, in this case, immediately and completely pervaded by the arsenic; for it appears, that the combination of these two metals, being extremely fusible, immediately flows, and is separated from the remaining part of the mass of gold, provided that the original degree of heat be not very speedily checked; but, when this happens, the mass or plate of gold remains coated with the arsenicated compound.

The effects which (according to these experiments) metallic arsenic appears to produce upon gold in a red heat, may in a great measure be compared to those which are observed when sulphur, or phosphorus, is combined with various metallic substances.

GOLD ALLOYED WITH ANTIMONY.

Experiment 1.

To eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, in perfect fusion, eighteen pennyweights and twenty-one grains of pure antimony were added, and, being well mixed, the whole was poured into a mould of iron.

This mixture had in some degree acted upon the surface of the mould; for it was with difficulty that the bar could be

removed; and, when this was effected, the internal surface of the mould appeared corroded in some parts, and, as it were, inlaid by the mixed metal.

Upon weighing the bar, it was found that only 15 pennyweights of the antimony remained combined with the gold; so that three pennyweights and twenty-one grains had been dissipated.

This metal was of a dull pale colour, not very unlike tutenague; it was excessively brittle, and in the fracture appeared of an ash colour, with a fine close grain, somewhat resembling that of porcelain.

Experiment II.

To eighteen pennyweights and ten grains of the fine gold, in fusion, nineteen grains of copper were added, which being melted and well mixed, nineteen grains of antimony were also added; after which, the metal was poured into a mould.

The external colour of the button was like gold made standard by copper; it was very brittle, and, in the colour and grain of the fracture, resembled the result of the preceding experiment.

Experiment III.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with one pennyweight and six grains of copper, and afterwards eight grains of antimony were added, to complete the standard proportion of alloy; the mixture was then poured, as expeditiously as possible, into a mould.

The ingot resembled that of the former experiment, in every particular, excepting that the grain of the fracture was more coarse, although it was still devoid of metallic lustre.

Experiment IV.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with one pennyweight and ten grains of copper, and, when in perfect fusion, four grains of antimony were added.

The external colour of the ingot was like that of *Experiments* II. and III. It was very brittle, and the grain of the fracture was similar to *Exper.* III. excepting that it shewed a small degree of metallic lustre.

Experiment V.

To one ounce sixteen pennyweights and twenty grains of the fine gold, alloyed with three pennyweights and three grains of copper, one grain of antimony was added, and the mixture was treated as before.

The antimony, in this mass, was in the proportion of only half a grain in each ounce; but the ingot was completely brittle, and the fracture still shewed a close grain, although the metallic lustre now began to be more apparent.

Experiment VI.

The two ounces of the metal formed by the preceding experiment, were added to two ounces of gold made standard by fine copper.

The proportion of antimony, in this experiment, could at most be estimated only at $\frac{1}{4}$ of a grain in the ounce; but, as it may be supposed that, by the repeated meltings, some of the antimony had been volatilized, it probably was in a less proportion.

The ingot formed by this experiment was, in colour, and in other properties, very like that of *Exper.* V. It was, however, in a

slight degree less brittle, as it did not so immediately break under the hammer.

The foregoing experiments prove, that $\frac{1}{4}$ of a grain of antimony in the ounce, or $\frac{1}{1920}$ part of the mass, can destroy the ductility of gold.

The following experiments were made to ascertain the effects of the vapour or fumes of antimony upon gold, when close and when open vessels were employed.

Experiment VII.

480 grains of fine gold were exposed, in close vessels, to the fumes of about 480 grains of antimony, under circumstances similar to those described in the third experiment upon arsenic.

When the crucibles were unluted, the chief part of the antimony was found, unchanged, at the bottom of the inferior large crucible.

The button of gold in the small crucible was not altered in the external colour, but proved to be extremely brittle, for it immediately split under the hammer, and exhibited a close grained earthy fracture, of an ash colour.

After the experiment, the button weighed 483,9 grs.; so that it had acquired 3,9 grs. of antimony.

Experiment VIII.

A small four-inch crucible, containing 480 grains of fine gold, was placed within another, of 12 inches; and four ounces of antimony were put into the large outer crucible, as soon as the gold appeared to be perfectly melted.

When half an hour had elapsed, the small crucible was

removed, and the gold was poured into a mould. The button was externally of a dull brownish colour, and was very brittle.

From the two last experiments, it may be inferred, that gold, when melted in close, and even in open vessels, attracts and combines with antimony in the state of vapour.*

GOLD ALLOYED WITH ZINC.

Experiment I.

Eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, were melted, after which, eighteen pennyweights and twenty-one grains of zinc were added, and, being quickly stirred, the whole was poured into a mould of iron.

Upon the addition of the zinc, a bright flame immediately arose; and, although as little time as possible was lost, yet, upon weighing the bar, it appeared, that in this short period, five pennyweights and twenty-one grains of zinc had been volatilized, and that only thirteen pennyweights remained combined with the gold.

The bar was of a pale greenish yellow, like brass, and was totally devoid of ductility.

Experiment II.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with 19 grains of copper, to which, when completely melted, 19 grains of zinc were added, and, being expeditiously mixed, the metal was poured into a mould.

The external colour of the ingot was pale yellow; it was

* It has been proved, that arsenic will not combine readily with gold in open vessels; but the reverse was observed when antimony, zinc, and some other metals, were reduced to vapour in the vicinity of melted gold. This effect appears to depend on the relative affinities of the different metals with gold and with caloric.

very brittle, and, like the former bar, exhibited a coarse grain in the fracture.

Experiment III.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with one pennyweight and six grains of copper, and, when in fusion, eight grains of zinc were added.

This experiment was conducted as quickly as possible; but nevertheless, upon weighing the ingot, it appeared that the whole of the zinc had been volatilized; and this was farther proved, by the colour, and by the perfect ductility of the metal.

Experiment IV.

To eleven ounces one pennyweight and three grains of the fine gold, in fusion, eighteen pennyweights and twenty-one grains of fine brass wire were added, and mixed as before.

The external and internal colour was of a fine pale yellow; but the metal was very brittle, and the grain of the fracture was coarse.

Experiment V.

To eighteen pennyweights and ten grains of gold, alloyed with 19 grains of copper, in fusion, were added 19 grains of fine brass.

This ingot did not, in general properties, differ from the former.

The following experiment was made, to ascertain the effects of zinc upon gold, when the two metals were melted in open vessels, near each other, without being in absolute contact.

Experiment VI.

One ounce of fine gold was melted in a four-inch crucible, which had been previously placed within another, of 12 inches.

As soon as the gold was in complete fusion, about four ounces of zinc were put into the large crucible.

A considerable flame, accompanied by a large quantity of white oxide of zinc, immediately arose, and part of the oxide adhered to the interior of the large crucible.

Within half an hour, the crucible containing the gold was removed, and was suffered to cool.

Upon examining the button, it appeared, that a portion of the volatilized zinc was combined with the gold; for the surface was dull, and of a Spanish snuff colour; moreover, it proved to be very brittle, similar to the former results.

From these experiments, it is evident, that zinc is highly injurious to the ductility of gold; that a portion of it is easily separated from gold by heat; that, when a large quantity of gold is alloyed with the standard proportion of zinc, only part of the latter is speedily volatilized, but, when small quantities are treated, the whole of the zinc becomes separated, and the gold remains pure; that, if zinc is previously combined with copper in the state of brass, it is not so easily separated by heat, when added to melted gold; and, lastly, that gold in fusion absorbs and retains a portion of zinc, when exposed to the latter metal in a volatilized state, even in open vessels.

GOLD ALLOYED WITH COBALT.

Experiment I.

The effects produced by cobalt upon gold, do not appear to have been hitherto investigated; for this reason, the following experiments were made.

To eighteen pennyweights and ten grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, when in fusion, one pennyweight and fourteen

grains of pure metallic cobalt were added, and, being well melted and mixed, the whole was poured into a dry cupel.

The external colour of this metal was a dull yellow; it was very brittle, and the fracture appeared of a pale yellow, with an earthy grain.

Experiment II.

To eighteen pennyweights and ten grains of the fine gold, alloyed with 19 grains of pure copper, were added 19 grains of metallic cobalt, which being perfectly melted and mixed, the whole was treated as before.

The metallic button, externally, appeared of a pale yellow, slightly tinged with gray; it was brittle, and shewed a fine-grained earthy fracture.

Experiment III.

Eighteen pennyweights and ten grains of the fine gold, alloyed with one pennyweight and six grains of copper, being melted, eight grains of cobalt were added, and mixed.

The colour of this ingot was like that of the former, but the yellow colour was rather deeper. It soon broke under the hammer, and the fracture was still of a fine grain, inclining to an earthy appearance.

Experiment IV.

To eighteen pennyweights and ten grains of gold, alloyed with one pennyweight and ten grains of copper, four grains of cobalt were added. The colour of this metal resembled that of gold made standard by copper, excepting that it was rather paler. This ingot was but slightly brittle.

As the last metal began to be ductile, the experiments with cobalt were not continued.

GOLD ALLOYED WITH NICKEL.

Experiment I.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with one pennyweight and fourteen grains of pure metallic nickel, and the whole was then poured into a cupel.

This button was of the colour of fine brass; it immediately broke under the hammer, with a coarse-grained earthy fracture.

Experiment II.

Eighteen pennyweights and ten grains of fine gold, being alloyed with 19 grains of copper, were afterwards melted, and mixed with 19 grains of pure nickel. The external colour of this metal resembled gold made standard by copper, but was paler in a slight degree. It was brittle, and shewed a fine-grained fracture, of an earthy appearance.

Experiment III.

Eighteen pennyweights of the fine gold, alloyed with one pennyweight and six grains of copper, being melted, eight grains of nickel were added, and mixed as before. The colour of the ingot was like that of the former experiment, and the metal proved to be only slightly brittle.

Experiment IV.

To eighteen pennyweights and ten grains of fine gold, alloyed with one pennyweight and ten grains of copper, when in fusion, were added four grains of nickel. The colour of this button was like that of gold made standard by copper, and, under the hammer and rollers, it was found to be perfectly ductile.

From these and the following experiments it appears, that, of all those which have been improperly called semimetals, nickel is that which is the least injurious to the colour and ductility of gold.

GOLD ALLOYED WITH MANGANESE.

From what is at present known, it does not appear that this combination has till now been made.

Experiment I.

480 grains of gold, 23 car. $3\frac{3}{4}$ grs. fine, being put into a crucible, were covered with about half an ounce of pure black oxide of manganese. The crucible was then exposed to a strong heat, in a wind furnace, during one hour and an half; but not any alteration was thus produced in the properties of the gold.

Experiment II.

A quantity of olive-oil was several times mixed, and burned, with some of the oxide of manganese, after which, about one ounce of the oxide was put into a crucible lined with charcoal.

A piece of fine gold, weighing one ounce, was then placed in the middle of the oxide, over which a stopper of charcoal was put, and the whole was closed by a cover, firmly luted.

After a strong heat of one hour and an half, the crucible was removed, and, when cold, was broken.

The manganese, in which the gold had been embedded, still remained in a pulverulent state, but, from black, was changed to a dark green.

The button of gold at the bottom of the crucible was of a pale colour; it soon broke under the hammer, and shewed a spongy coarse-grained fracture.

Experiment III.

From the effects last mentioned, it was evident that manganese could be thus combined with gold; the experiment was therefore repeated, and the heat was continued during three hours, at the end of which time the crucible began to be melted.*

Upon breaking the crucible, which had been suffered to cool in the furnace, the manganese was found to be pulverulent in some parts, and indurated in others. There were not any metallic globules to be seen; and the colour varied from dark to pale grass green.

The button of gold at the bottom of the crucible was uniform, and externally of a pale yellowish gray colour, with a considerable lustre, almost equal to that of polished steel.

On that part of the button which had been next to the bottom of the crucible, were some specks of pale green enamel. The metal possessed a small degree of ductility, although extremely hard, for, when placed upon an anvil, being repeatedly struck with a heavy hammer, the button was in some measure flattened, before it could be broken.

The fracture was coarse, very spongy, and of a reddish gray colour; and many cavities, in the interior of the mass, were filled with the dark green coloured manganese.

It has been generally observed, that metals, when combined with manganese, are liable to a speedy change and diminution of lustre, colour, &c. when exposed to the air; but gold alloyed

* This reduction and union of manganese with gold, seems to have been effected by the double affinities between oxygen, carbon, gold, and manganese; and there is every reason to believe, that the above method may be advantageously employed, to form the alloys of the refractory metals with those of easy reduction and fusibility.

in the manner abovementioned, did not suffer (even when several months had elapsed) any perceptible alteration.

Mr. BINGLEY, who was always obligingly ready to assist in these experiments, at my request, examined the habits of this combination of manganese with gold, by the usual process of assaying. He found that the manganese was completely metallized, and combined with the gold, but not in an exactly equal proportion throughout the mass; for, in one part the manganese amounted to $\frac{1}{8}$, and in another to $\frac{1}{9}$.

The gold defends the manganese, in the metallic state, from the action of those acids which usually dissolve it.

When the mixed metal is exposed to a great heat, with free access of air, it loses its metallic lustre, and is covered with a dark brown oxide.

24 carats of the metal, which had been exposed to a considerable heat under a muffle, acquired $\frac{1}{19}$ of its weight.

Another time, under similar circumstances, it acquired $\frac{1}{16}$ of its original weight; but this proportion of oxygen disposed it to vitrify, and the mass was fixed to the cupel by a dark blue enamel.

Nitric or sulphuric acid alone, cannot completely dissolve this oxide; but, a little sugar being added to the nitric acid, enables it superficially to dissolve the oxide, and to separate it from the gold, which then remains clean, and of its natural colour; yet the manganese has only thus been removed from the surface, for, when the mass is cut, the interior exhibits the original gray colour of the mixed metal.

The gold may be purified from the manganese by lead alone, if there is heat and lead sufficient; but this may be more completely and certainly performed, by the mixture of silver, and separation by nitric acid.

As soon as the fine metals are in fusion, the manganese floats upon the surface of the lead, is gradually vitrified, and is absorbed by the cupel, which becomes tinged with a dark blackish-brown colour throughout the whole of its substance; and it must be observed, that this tinge is very different from that afforded by any other metal with which gold may be alloyed.

The cupels on which the manganese has been separated from gold by lead, are corroded in deep holes, by the compound of manganese and lead; an effect which is never observed when copper, or any other of the alloys of gold are destroyed by lead.

The mixture of gold and manganese is more difficult of fusion than gold alone; yet, when the heat is continued with access of air, the whole of the manganese becomes oxidized, and remains on the surface; and, when the mass is cold, it may be separated from the pure gold which is underneath, by the blow of a hammer.

24 carats of the mixed metal, which had acquired $\frac{1}{19}$ of its weight by the absorption of oxygen, was reduced in close vessels, by fusing it with charcoal and oil, as already described.

The manganese again became metallized, and was again combined with the gold, to which it communicated the gray colour and brittleness of the original metal; and the button weighed nearly as at first, viz. 24 carats.

The solution of the mixed metal in nitro-muriatic acid, when evaporated to dryness, leaves a light or pale orange-coloured spongy mass, not so readily deliquescent as the evaporated solutions of pure gold, which are also of a much deeper colour.

The solution of the mixed metal affords, by ammonia, a mixed precipitate, composed of yellow and white particles; but

the mixture of manganese, in this precipitate, does not appear to diminish, or in any way affect, the fulminating property of the gold.

GOLD ALLOYED WITH BISMUTH.

Experiment I.

To eighteen pennyweights and ten grains of gold, 23 car. 3½ grs. fine, when in perfect fusion, were added one pennyweight and fourteen grains of pure bismuth.

The external colour of the metal was pale greenish yellow, like that of bad brass.

It immediately broke under the hammer, and shewed a fine-grained earthy fracture.

Experiment II.

Eighteen pennyweights and ten grains of the fine gold, were alloyed with one pennyweight and six grains of copper, after which, eight grains of bismuth were added.

The ingot appeared, externally, of a pale brownish yellow; it was very brittle, and the fracture was like that of the former, excepting that the grain was not so fine.

Experiment III.

Eighteen pennyweights and ten grains of fine gold, were alloyed with one pennyweight and ten grains of copper, and four grains of bismuth were then added, to complete the standard proportion of alloy.

This metal was, in colour, like gold made standard by copper; it was very brittle, but the grain was coarse.

Experiment IV.

To eighteen pennyweights and ten grains of gold, alloyed with one pennyweight and thirteen grains of copper, one grain of bismuth was added.

The colour of the metal was like that of *Exper.* III. it was very brittle, and, in the fracture, shewed a much coarser grain.

Experiment V.

Eighteen pennyweights of the fine gold, alloyed with one pennyweight and fourteen grains of copper, being completely melted, the button formed by *Exper.* IV. was added, and mixed.

In the two ounces of metal, one grain only of bismuth was present; nevertheless, the ingot was extremely brittle, and the grain of the fracture was remarkably coarse and spongy.

Experiment VI.

To eighteen pennyweights and ten grains of fine gold, alloyed with one pennyweight and fourteen grains of copper, when in fusion, one ounce of the metal formed by *Exper.* V. was added. There now was, at most, not more than $\frac{1}{4}$ of a grain of bismuth in each ounce; but the metal was still brittle, although rather in a less degree than before.

The grain was not spongy; and, as well as the colour, was similar to gold made standard by copper.

From these experiments it appears, that $\frac{1}{4}$ of a grain of bismuth in one ounce Troy of standard gold, or $\frac{1}{1920}$ of the mass, is capable of destroying all ductility; and there is reason to believe, that even a smaller quantity would produce a considerable effect.

The following experiments were made, to ascertain the effects of the fumes or vapour of bismuth upon gold, when melted in close, and when in open vessels.

Experiment vii.

One ounce of the fine gold, being put into a small four-inch crucible, was placed within a large one of about 12 inches, and another large crucible, inverted, was then fixed and luted, in the manner of a dome. One ounce of bismuth was previously put into the inferior large crucible, on the outside of that which contained the gold, after which, a strong heat was kept up during two hours.

Upon opening the vessels, the bismuth was found in a mass, at the bottom of the large crucible; but a considerable part had been volatilized; for the button of gold, which before the experiment weighed 480 grains, now weighed 512,2 grs. and had therefore acquired 32,2 grs. of bismuth. It was, externally, of a pale brassy colour, and immediately split under the hammer, with a coarse-grained fracture.

Experiment viii.

The preceding experiment was repeated, but without the upper large crucible or dome, so that a free circulation of air was admitted. When half an hour had elapsed, the crucible containing the gold was removed.

The external colour of the button was not altered; but it proved to be so brittle, that it was immediately broken by the first blow of the hammer.

The whole of the foregoing experiments concur to prove, that bismuth, under all circumstances, readily combines with gold, and that it is most exceedingly injurious to the ductility of it.

GOLD ALLOYED WITH LEAD.

Experiment I.

To eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, eighteen pennyweights and twenty-one grains of pure lead were added, and, being well mixed, the whole was poured into a mould of iron.

The external colour of the bar was like fine gold, but rather more pale; it had not the smallest degree of ductility, for it broke like glass. The grain was very fine, and of a pale brown colour; it was devoid of metallic lustre, and had a porcellaneous appearance.

Experiment II.

To eighteen pennyweights and ten grains of the fine gold, in fusion, 19 grains of copper were added, and, when the copper was melted and well mixed, 19 grains of lead were also added, to complete the standard proportion of alloy; the metal was then poured into a dry cupel.

The button was, in external colour, like gold made standard by copper; but it immediately broke under the hammer, with an earthy fracture; the grain, however, was not so close as that of the former experiment.

Experiment III.

Eighteen pennyweights of fine gold, were alloyed with one pennyweight and six grains of copper, after which, eight grains of lead were added.

The colour of the metal was like that of the former, but it was perfectly brittle.

The grain of the fracture was rather coarse.

Experiment IV.

Eighteen pennyweights and ten grains of the fine gold, were first alloyed with one pennyweight and ten grains of copper, and afterwards with four grains of lead. This, in colour, resembled the former; it was also as brittle, but the grain was coarser, and shewed some metallic lustre.

Experiment V.

In *Experiments* II. III. and IV. one ounce of standard gold had been prepared, but two ounces were now employed; so that the lead should be in the proportion of one grain to each ounce.

To one ounce sixteen pennyweights and twenty grains of fine gold, when in fusion, three pennyweights and two grains of copper were added, and afterwards two grains of lead.

This ingot resembled the former, and was exceedingly brittle.

Experiment VI.

The ingot formed by the preceding experiment was melted again, with one ounce sixteen pennyweights and twenty grains of fine gold, alloyed with three pennyweights and four grains of copper, so that the lead was in the proportion of ½ a grain in each ounce

The colour was as before; and the metal was very brittle, and of a very loose spongy texture. The latter circumstance explains why the specific gravity amounted only to 16.627.

Experiment VII.

One ounce of the metal formed by *Exper.* VI. was melted with eighteen pennyweights of fine gold, and one pennyweight

and fourteen grains of copper, so that, at most, there could only be $\frac{1}{4}$ of a grain of lead in the ounce.

The colour of this metal resembled the former; and it was also very brittle; but the texture was not spongy, for it was very similar to that of gold made standard by copper.

Experiment VIII.

One ounce of the fine gold, in a small four-inch crucible, was placed within another of 12 inches, at the bottom of which, about two ounces of lead were put on the outside of the small crucible. An inverted crucible, of 12 inches, was then luted on, and the whole was exposed to a strong heat, in a wind-furnace, during two hours.

When the vessels were cold, and were opened, the colour of the gold in the small crucible was found to be unchanged; but, instead of weighing 480 grains, it now weighed 488,1 grs. and was so brittle, that it was immediately broken by the hammer. As so large a quantity of volatilized lead had thus been combined with gold, in close vessels, the following experiment was made with the free access of air.

Experiment IX.

One ounce of fine gold, in a small crucible, was placed within another of 12 inches, containing about four ounces of lead, which, remaining open, was exposed to a strong heat in a wind-furnace, during half an hour.

The gold in the small crucible was afterwards examined; and the external colour was found to be but little changed; the ductility of it was also not much injured, for, when repeatedly hammered, it only cracked slightly on the edges.

These experiments with lead, prove how destructive it is to the ductility of gold; and that the properties of it, in this and many other respects, much resemble those of bismuth, excepting that the latter has a more powerful effect, when reduced to vapour in the vicinity of melted gold.

Bismuth, as well as lead, when combined with gold in certain proportions, produces some remarkable effects upon the texture and specific gravity of the latter metal; and these effects are the most conspicuous, when either of the former metals is in the proportion of half a grain in the ounce Troy of gold alloyed with copper; for then the alloyed gold becomes remarkably spongy, and suffers a very considerable diminution of specific gravity.

GOLD ALLOYED WITH TIN.

Experiment 1.

Eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, being melted, eighteen pennyweights and twenty-one grains of pure grain tin were added, and, when well mixed, the whole was poured into a mould of iron.

The bar was in some measure brittle; but, as this appeared to be partly caused by the thickness, ($\frac{1}{4}$ of an inch,) the experiment was repeated as follows.

The same bar was melted again, and was cast in sand, so as to form a bar only $\frac{1}{8}$ of an inch in thickness.

This appeared to bend so easily, that it was passed between the rollers, but it then broke longitudinally into several pieces.

The external colour of the bars, in both experiments, was very pale whitish yellow.

The grain of the fracture was fine, inclining to an earthy appearance, and was of a pale yellowish-gray colour.

Experiment III.

Eighteen pennyweights and ten grains of fine gold, alloyed with 19 grains of copper, being melted, 19 grains of fine grain tin were added, and, being properly mixed, the metal was poured into a cupel.

The button was, externally, pale yellow; and it soon broke under the hammer, with a close-grained and rather earthy fracture.

Experiment IV.

To eighteen pennyweights and ten grains of the fine gold, alloyed with one pennyweight and six grains of copper, in fusion, eight grains of pure tin were added, and treated as before.

This metal resembled, in colour, gold made standard by copper, excepting that it was rather paler; it also proved to be perfectly ductile.

Two pounds of gold were afterwards alloyed with tin and copper; the former was in the proportion of eight grains in the ounce, similar to the last experiment; and, in like manner, the alloyed metal was found to be perfectly ductile; so that the bar, which at first was $\frac{1}{4}$ of an inch in thickness, was rolled as thin as a guinea, and, when thus rolled, it still remained so soft, that it was punched and stamped without being previously annealed.*

* It has been suggested, that tin might be advantageously employed as an alloy for silver coin; but, by some experiments which I purposely made, I found the fact to be the reverse; for, when silver was alloyed with the standard proportion of tin, it proved brittle, and did not ring well; and the same defects prevailed, when an alloy composed of equal parts of tin and copper was employed.

It is therefore certain, that tin, in small quantities, or even in the proportion of eight grains in the ounce Troy, is not by any means so injurious to the ductility of gold as was generally believed, previous to the publication of Mr. ALCHORNE's Paper, in the Philosophical Transactions for the year 1784.*

Some time after the experiments lately described were made, ROBERT BINGLEY, Esq. his Majesty's Assay Master at the Mint, communicated to me the following experiments, made by him, in consequence of a paper published in the Memoirs of the Academy of Sciences at Paris for the year 1790, and in NICHOLSON's Philosophical Journal, Vol. II. p. 140 and 179.

The author of this paper, Mr. TILLET, a gentleman of much eminence in science, after having related some experiments which he purposely made upon mixtures of gold and tin, states it as his opinion, that tin, in small quantities, is really injurious to the ductility of gold, especially when gold thus alloyed is subjected to what he terms a cherry-red annealing heat; and this circumstance, he conceives to have been overlooked by Mr. ALCHORNE.

Mr. BINGLEY, whose professional accuracy is sufficiently

* GELLERT asserts, that even the fumes of tin destroy the ductility of gold. Metallurgic Chemistry, p. 368. The same opinion may also be found in the following

Docimasie de SCHLUTTER, *traduit par* HELLOT, p. 284. *E'lémens de Docimastique par* CRAMER. Tome I. p. 142. NEUMANN's Chemistry, Vol. I. p. 49; and, in page 125 of the same volume, he says, "the minutest portion, even the vapour of "tin, renders many ounces, and even pounds, of gold and silver, so brittle as to fall in "pieces under the hammer The least particle of tin falling on the stones or luting "of a furnace, will make all the gold and silver melted in it hard and brittle. From "such an accident, the gold and silversmiths are obliged to pull down the whole "furnace, and build a new one with fresh materials."

known, therefore made the following experiments, in order to investigate the subject in dispute.

MR. BINGLEY'S EXPERIMENTS UPON GOLD ALLOYED WITH TIN.

I.

" Two bars of gold alloyed with tin, in the proportion of " eight grains in the ounce Troy, and which had passed the " steel rollers without any disposition to break on the edges, " were submitted to an annealing heat, under a muffle, at two " different temperatures.

" The first bar was exposed to a low degree of heat, visibly " red by daylight, which contracted WEDGWOOD'S pyrometer " five degrees.

" At this temperature, the metal was sufficiently annealed, " had lost the sonorous property acquired by passing the rollers, " was quite ductile, and capable of being worked into any form; " and was slightly discoloured or oxidized on the surface.

" The second bar was subjected to a somewhat greater heat, " approaching the cherry-red described by Mr. TILLET, and " which contracted the pyrometer ten degrees.

" At this temperature, some sensible changes soon began to " take place; and, by a constant attentive watching of the metal " during this exposure, three gradations of mischief were evi- " dently marked.

" 1st. Little distinct bubbles or blisters arose in different " places on the surface.

" 2dly. The whole bar, in a short time, began to curl up, or " warp, on the edges. And,

" 3dly. When the whole of the tin, diffused through the " interior of the metal, might be supposed to be in fusion, a

" solution of continuity followed; for the bar, by its own weight, " fell from the supporters on which it was placed, in a rough " dark-coloured mass, having scarcely any appearance of metal, " although it recovered its metallic lustre, and some tenacity, by " being hammered upon a polished anvil.

II.

" These experiments were repeated, with exactly the same " results; and seem to prove, beyond a doubt, that gold alloyed " with tin may be repeatedly annealed, after passing the rollers, " without any danger, provided that due attention be paid to " the temperature to which it is exposed.

" Hence it may fairly be said, that all which Mr. TILLET has " advanced on the foregoing subject, if closely attended to, will " be found to go in confirmation of what Mr. ALCHORNE had " previously asserted, relative to the mixture of gold with tin; " and Mr. TILLET only adduces the additional fact, of a greater " degree of heat destroying the union of the two metals, which, " from a full consideration of their peculiar properties, and dif- " ferent fusibility and specific gravity, might have been inferred " *a priori*. It is also reasonable to suppose, that whenever a " combination of a more with a less fusible metal takes place, " a temperature insufficient to fuse the whole mass would tend " to separate the one from the other; and this is the principle " on which the process of eliquation depends.

" Mr. ALCHORNE was induced to make the experiments on " tin and gold, from reading the following passages. 'A single " grain of tin will destroy the ductility of a thousand grains of " gold, rendering the most malleable gold incapable of being " extended, and of bearing the hammer at all.' NEUMANN'S " Chemistry, Vol. I. p. 49.

" 'Of tin and lead, the most minute portions, even the va-
" pours which rise from them in the fire, though not sufficient
" to add to the gold any weight sensible in the most delicate
" balance, make it so brittle, that it flies to pieces under the
" hammer.' LEWIS's Philosophical Commerce of Arts, p. 85."

There must have been, undoubtedly, some cause from whence originated the general and universal opinion, that tin was so injurious to the ductility of gold, although it is now evident that this opinion is, within certain limits, erroneous; and it appears to me very probable, that the chemists and metallurgists who first promulgated the idea that the ductility of gold was destroyed even by the fumes of tin, made their experiments with tin which contained a small portion of bismuth, lead, antimony, or zinc; and, as the foregoing experiments have proved, that even less than $\frac{1}{1920}$ of the three first of these metals is sufficient to make gold very brittle, and that gold in fusion attracts and combines with them, when in a state of vapour, we may suspect these to have produced the principal part of the effect which has been attributed to tin, and which has been so generally asserted and believed.

As to the difference of opinion between Mr. ALCHORNE and Mr. TILLET, it seems that both these gentlemen have just grounds for their assertions; and that the different degree of annealing heat, employed by the one and by the other, has been the cause, why the results of their experiments appear to be so opposite.

GOLD ALLOYED WITH IRON.

The effects of iron upon gold seem to have been less understood, and more erroneously stated, than even those of tin. For

it has been so repeatedly asserted by various chemists, and so universally believed by all persons concerned in the melting and working of gold, that the ductility of it is destroyed, or much injured, by very small portions of iron, that, had it not been for the determination made, when these experiments were begun, to examine, by experiments made expressly for the purpose, every assertion respecting the effect of alloys, this long established opinion would not here have been questioned.

Indeed, considering how easy it was to ascertain whether or not gold could be rendered brittle by the addition of iron, it is a matter of astonishment, when we read in the works of many celebrated chemists, and when we hear intelligent and judicious artists assert, that iron, even in minute proportions, injures or destroys the ductility of gold.*

It is not necessary here to inquire how this erroneous idea first originated; but, that the fact is absolutely the reverse, the following experiments will sufficiently prove.

Experiment 1.

To eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ fine, eighteen pennyweights and twenty-one grains of clean iron wire were added.

The iron was soon melted, and was well mixed with the gold, after which, the whole was poured into a greased mould of iron.

* "Le fer qui y touche, (l'or,) quand il est en fusion, *l'aigrit aussi,* au lieu qu'il adoucit l'argent." SCHLUTTER, p. 284.

"Iron or steel, in a very small proportion, render gold hard and eager, and, on "increasing the quantity of the iron, the mixt continues *brittle.*" LEWIS's Phil. Comm. of Arts, p. 85.

"When gold is fused with iron, it is by it rendered pale
Principles of Modern Chemistry, Vol. II. p. 339.

The bar thus formed was of a pale yellowish gray, approaching to a dull white; it was very ductile, and, with great ease, was reduced by the rollers from $\frac{1}{4}$ of an inch to the thickness of a guinea. It was then cut without difficulty, by the punches, into blanks, and these were afterwards stamped with great ease, although they had not been annealed.

Experiment II.

The same bar was again melted, and was cast in a sand mould; it then appeared to be very brittle, but this, it was proved, was occasioned by the effects of the sand mould, which had rendered the metal porous; for, when the parts had been approached by previous hammering, the bar was rolled, and, in every respect, was found to be as ductile as in the former experiment.

Experiment III.

Eighteen pennyweights and ten grains of the fine gold being melted, one pennyweight and fourteen grains of cast-iron nails were added, and, when fused, the whole was cast in a mould of iron.

This metal, in colour, much resembled the former; and was also perfectly ductile.

Experiment IV.

This was the same as *Exper.* III. but thin plate cast-steel was employed, instead of cast-iron.

The properties of this metal, in respect to colour, ductility, &c. resembled the former.

Experiment V.

To eighteen pennyweights and ten grains of fine gold, alloyed

with 19 grains of copper, 19 grains of fine iron wire were added, and the mixture was treated as before.

The colour of this metal was pale grayish yellow; it was perfectly ductile, and was rolled and stamped, without being previously annealed.

By these experiments it appears, that gold made standard by wrought and cast iron, and even by cast-steel, is not brittle, as has generally been asserted; for, although gold undoubtedly is thus rendered harder, it nevertheless does not become brittle, but remains so ductile, that it may be hammered, rolled, and stamped, without requiring to be annealed; and, allowing that the change of colour produced by iron upon gold renders it unfit for coin, yet this mixture may probably be employed with advantage in ornamental and other works.*

Emery is enumerated by mineralogists among the ores of iron; and many very intelligent assayers, and others, even at this time, believe it to be a frequent cause of the brittleness of gold.

Some eminent metallurgists, such as SCHLUTTER, support the same opinion, and have recommended certain processes to be employed to refine the gold, when thus adulterated.† It must however be allowed, that it is not easy to conceive how such a combination can take place; for if (as is generally believed) emery consists of oxide of iron and siliceous earth, such a substance cannot unite with gold in the metallic state; and, even supposing that the ferruginous ingredient could in any manner be combined with gold, yet it has been fully proved, in another

* It is said that this mixture is sometimes employed by goldsmiths.

† SCHLUTTER, *Docimasie*, p. 282.

part of this Paper, that gold does not become brittle by the addition of iron. In order, however, to ascertain what effects could be produced by emery,* the following experiments were made.

EXPERIMENTS ON EMERY AND GOLD.

Experiment I.

One ounce of fine gold was put into a small crucible, and was completely covered with emery, which had been reduced to a fine powder.

The gold was kept in fusion during one hour, and was frequently stirred, after which, it was poured into a mould.

Not the smallest change, in colour, ductility, or any other property, was thus produced.

Experiment II.

About half an ounce of fine powder of emery was, several times, alternately moistened with olive oil and made red hot, after which, it was put into a crucible lined with charcoal.

* Mr. TENNANT has lately shown, (Phil. Trans. for 1802, page 400,) that emery is composed of alumina, silica, and iron. In one case, he obtained,

Alumina	80
Silica	3
Iron	4
Residuum	3
	90.

And, from another variety, more impregnated with iron, he obtained,

Alumina	50
Silica	8
Iron	32
Residuum	4
	94.

with 19 grains of copper, 19 grains of fine iron wire were added, and the mixture was treated as before.

The colour of this metal was pale grayish yellow; it was perfectly ductile, and was rolled and stamped, without being previously annealed.

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Iron - - - -	4
Residuum - - -	3
	90.

And, from another variety, more impregnated with iron, he obtained,

Alumina - - -	50
Silica - - - -	8
Iron - - - -	32
Residuum - - -	4
	94.

An ounce of fine gold was placed in the middle of the emery; the crucible lined with charcoal was closed by a stopper of the same, and a cover was luted upon the exterior crucible.

After a very strong heat of one hour and a half, the gold was found to be exactly of the same colour, ductility, &c. as before.

Experiment III.

The preceding experiment was repeated; but the heat was continued during three hours, so that at length the crucible began to be melted.

It was suffered to cool in the furnace; and, being afterwards broken, the gold was found crystallized in a reticulated form, but not altered in colour or ductility. The emery was reduced to a dark gray or blackish slag, which occupied the upper part of the crucible.

We have, from the above described experiments, sufficient proof that emery will not combine with gold; and, when the difficulty of uniting metallic oxides in general, or any earthy substance, with a metal, is considered, it appears singular that the existence of such a combination as that of emery with gold should ever have been believed.

It is not however improbable, but that some other substance [illegible] occasionally denoted by the term smiris, emeril, or [illegible] appears to be inclined to adopt this opinion.*

GOLD ALLOYED WITH PLATINA.

Experiment I.

[illegible] weights and ten grains of gold, 23 car. $3\frac{1}{2}$ grs.

* [illegible] Comm. of Arts, p. 607.

fine, being completely melted, one pennyweight and fourteen grains of purified platina were added, and, when well mixed, the whole was poured into a mould.

This metal was of a yellowish white, like tarnished silver, and was extremely ductile.*

The specific gravity of it was 19,013.

Experiment II.

The ounce of standard gold formed by the foregoing experiment, was again alloyed with one pennyweight and fourteen grains of copper, so that the standard proportion of alloy was doubled.

This metal was of a pale dull yellow; it was not quite so ductile as the former; and the specific gravity was 16,816.

It was not thought necessary to extend these experiments, as the properties and effects of platina upon gold are now so generally known, as well as the means usually employed to detect it.†

* The platina had been purified by precipitation with muriate of ammonia, and was therefore in the state of a fine metallic powder, which probably contributed much to facilitate the union of it with the gold. I must observe, however, that it was not absolutely pure, for it still contained a small portion of iron.

The specific gravity of this platina was 18,717.

Gold made standard by platina, is not only very ductile, but also (when hammered) tolerably elastic. Perhaps it might be advantageously employed for the springs of watches, &c.

† Quand le platine ne surpasse pas les 30 à 40 millièmes de son alliage avec l'or, ce dernier n'en garde point, si le départ est fait avec les précautions nécessaires; et, lorsque ce métal est au-dessus de ce terme, la fraude devient trop sensible et trop évidente, pour qu'on ne s'en apperçoive pas; 1mo, par la plus grande chaleur que l'essai demande pour passer et prendre une forme arrondie; 2do, par l'absence de l'éclair; 3tio, par la surface crystallisée, et la couleur blanche et matte du bouton; 4to, par la couleur jaune de paille, plus ou moins foncée, qu'il communique à l'eau-forte pendant

GOLD ALLOYED WITH COPPER.

Experiment I.

Eleven ounces one pennyweight and three grains of gold, 23 car. $3\frac{1}{2}$ gr. fine, were alloyed with eighteen pennyweights and twenty-one grains of the finest Swedish copper.

The heat employed was only just sufficient to keep the whole in fusion; and, when the alloy appeared to be well mixed, the metal was poured into a greased mould of iron.

The standard gold, thus formed, was perfectly ductile, and of a deep yellow colour, inclining to red.

Experiment II.

The bar formed by the preceding experiment was melted in the strongest heat which could be excited, and was again cast in the mould of iron.

Not any alteration, however, appeared to have been produced by the increased heat.

Experiment III.

The bar was again melted, and was then cast in sand; but still the ductility and other properties remained as before.

From these experiments it appears, that gold made standard [illegible] does not suffer any change in ductility by the dif[illegible] nor by the nature of the moulds in which the [illegible] is [illegible] that the copper be pure.

Experiment IV.

[illegible] ounces one pennyweight and three grains of fine gold,

[illegible]part; 5mo, enfin par la couleur jaune pâle, et tirant au blanc, du cornet, quand [illegible]. *Manuel de l'Essayeur, par* VAUQUELIN, p. 49.

were alloyed with eighteen pennyweights and twenty-one grains of granulated Swedish copper, which was taken from another parcel.

The bar was cast in a mould of iron, and was found to be, in a small degree, less ductile than the metal formed by the foregoing experiments.

Experiment V.

The same bar was melted again, and was cast in sand, faced in the usual manner by charcoal dust; it then proved slightly brittle.

Experiment VI.

The bar was melted as before, and was cast in the mould of iron.

The metal was now found to have recovered the same ductility which it possessed in *Exper.* IV.

Experiment VII.

Eleven ounces one pennyweight and three grains of fine gold, were alloyed with eighteen pennyweights and twenty-one grains of Swedish dollar copper, which being well mixed, the whole was cast in a mould of iron.

The colour of this bar resembled the former; but it did not possess the smallest degree of ductility, for it broke like glass.

Experiment VIII.

The bar was melted again, and was cast in sand; but not any alteration was thus produced.

Experiment IX.

The foregoing bar was melted once more, and was cast in the mould of iron; but it still remained as brittle as at first.

Experiment x.

One pound Troy of standard gold was made as already mentioned, and another Swedish copper dollar was employed for the alloy.

The metal was cast in a mould of iron, and proved to be ductile; it is therefore very evident that the Swedish copper dollars are not of an uniform quality.

Experiment xi.

Eleven ounces one pennyweight and three grains of the fine gold, were alloyed with eighteen pennyweights and twenty-one grains of fine British granulated copper.

When this was cast in a mould of iron, it was found to be slightly brittle.

Experiment xii.

When the same bar was cast in sand, the brittle quality was much increased.

Experiment xiii.

Eleven ounces one pennyweight and three grains of fine gold, were alloyed with eighteen pennyweights and twenty-one grains of British granulated copper which was considered as better than the former.

The bar was cast in the mould of iron, and was very ductile.

Experiment xiv.

The same bar being melted, was cast in sand, and then was found to be brittle.

Experiment xv.

The above-mentioned brittle bar was again melted, and was cast in the mould of iron.

It then became ductile, but not quite in so great a degree as in *Exper.* XIII.

Experiment XVI.

Six ounces of fine gold, and six ounces of the finest Swedish copper, were melted, and mixed. This metal, being cast in the mould of iron, was ductile.

Experiment XVII.

The bar was melted again, and, being cast in sand, was then found to be very brittle.

From these experiments it appears, that the varieties of copper in commerce, although similar in aspect, and other obvious properties, are far from being uniform in quality; so that many of them are by no means sufficiently pure to be employed as an alloy for gold.

Moreover, the different effects produced by the moulds of iron and those of sand, are such as fully prove, that copper which is not perfectly pure, and which has a tendency to render gold brittle, acts more powerfully, in this respect, when the alloyed mass is cast in sand than when it is cast in iron; and, all things being considered, we have reason to conclude, that moulds of iron are much to be preferred to those of sand.*

The ores of antimony and of lead frequently accompany those of copper; and it has already been proved, that $\frac{1}{1920}$ of either

* Bars of alloyed gold (particularly those which are alloyed with copper) are generally discoloured on the surface, when cast in moulds of sand; but not so when cast in iron. It may be suspected, that the alloy is superficially oxidized when sand is employed, in consequence of the air which is lodged in the interstices, together perhaps, with some degree of moisture.

of the former metals is sufficient to destroy the ductility of gold. It may therefore be suspected, that the brittle quality which certain kinds of copper communicate to gold, proceeds from those metals; for, although other metallic substances produce the same effect, yet, as the former especially are so commonly present with the ores of copper, it is highly probable that antimony, or lead, may remain combined with the smelted copper, in a proportion too small to affect the general and more obvious properties of that metal, yet still sufficient to destroy the ductility of gold, when such copper is employed as an alloy.

To ascertain how far copper might be alloyed with lead, or antimony, without any very apparent change in its obvious properties, the following experiments were made.

To 476 grains of fine malleable copper, in fusion, four grains of antimony were added, and, being well mixed, the whole was poured into a mould.

The colour of this copper, when filed and polished, was such as not easily to be distinguished from that which had not been thus alloyed.

It was also hammered and rolled, without shewing any signs of brittleness. The specific gravity was 8,354.*

The like quantity of copper was alloyed with four grains of lead. [illegible] ductile, and did not suffer any apparent change [illegible].

The specific [illegible].

The same experiment was repeated with four grains of bismuth; [illegible] thus alloyed was exceedingly spongy and [illegible].

* The finest [illegible] was employed in these experiments. The specific

It appears, therefore, that four grains of antimony, or of lead, may be present in one ounce, or 480 grains, of copper, without producing any very apparent change in colour or ductility, and but little in specific gravity; such copper may, therefore, without suspicion, be occasionally employed to alloy gold; then, however, the antimony or lead will produce a powerful effect; for it has been proved, that $\frac{1}{1920}$ of either of these will destroy the ductility of gold. But, supposing one ounce Troy of copper which contains four grains of antimony, or of lead, to be employed to alloy eleven ounces of gold, 24 carats fine, there would then be four grains of the abovementioned metals in the 12 ounces or Troy pound; and therefore the quantity of these would be considerably more than is required to destroy the ductility of gold. For the Troy pound contains 5760 grains; and 4 is to 5760 as 1 to 1440; consequently, this proportion much exceeds the quantity which is capable of producing the abovementioned effect.

But the copper of commerce often contains a much greater proportion of one or other of these metals; and, although it then appears more pale than common, yet it has, without suspicion, been purchased by those who, from their profession, are supposed to be competent judges, and who especially require copper to be as pure as possible. Persons of this description, however, are liable to be deceived; for, in 1791, Mr. ROITIER, Director of the Mint at Paris, purchased a quantity of copper from the mines of Poullaoen in Britany; but he soon discovered, from the effects which it produced, when employed as an alloy, that it was not pure, and therefore requested Mr. SAGE to examine it. By the latter, it was analysed, and was found to contain one forty-eighth of antimony.*

* Journal de Physique, 1792, Tome XL. p. 27[illegible].

Allowing, therefore, that other metallic substances may at times be present in copper, and may contribute to affect gold which is alloyed with it, yet, for the reasons above related, I am inclined to attribute, most frequently, this effect to antimony or lead.*

* Copper which is pure, is uniform in its effects, and does not injure the ductility of gold; it would therefore be proper, in all cases when copper is to be purchased for the purpose of alloying gold, to make a previous trial of it on a small quantity, as this would answer every purpose of a tedious and expensive analysis.

Since the above was written, I have made various experiments in the humid way, on the different kinds of copper which are known in commerce, especially on the following:

No. 1.	Finest granulated Swedish copper - -	sp. grav. 8,895.
2.	—— Swedish dollar copper - - -	sp. grav. 8,799.
3.	—— sheet British copper - - -	sp. grav. 8,785.
4.	Fine granulated British copper - - -	sp. grav. 8,607.

480 grains of the first, only afforded a few particles of sulphate of lead, which could not be estimated.

The second contained both lead and antimony, of which the lead was in the largest proportion, as it amounted to nearly one grain of metallic lead, whilst the antimony did not exceed half a grain.

The sheet British copper yielded some lead, with scarcely any antimony; and, on the contrary, the granulated British copper contained antimony with but very little lead. We may therefore conclude, that the varieties of copper known in commerce, are seldom, if ever, absolutely free from lead or antimony; and that the brittle quality, so [illegible] communicated to gold by an alloy of copper, arises from the presence of [illegible], which, even in the proportion of [illegible] part of the mass, I [illegible] of destroying the ductility of gold.

I have lately made some further inquiries concerning the varieties of Swedish copper, and am informed, that the fine granulated copper is made in this country from [illegible] copper, merely by the ordinary process of granulation; and, as the quality [illegible] has been found variable, the Deputy Master of the Mint has of late employed British copper, which has been refined expressly for the purpose, and seems to answer perfectly well. Respecting the variable and occasional very [illegible] of the copper dollars, Mr. SWEDENSTIERNA, a learned Swedish gentleman

GOLD ALLOYED WITH SILVER.

Gold alloyed with pure silver, in standard proportion, is so generally known, that it would be needless here to say more, than that it approaches the nearest to the ductility of fine gold, and that the specific gravity of this mixture differs but very little from that which, according to calculation, would result from the relative proportions of the two metals.

From the foregoing experiments it is evident, that many of the metallic substances with which gold may be alloyed, are more or less liable to be separated from it during fusion, in consequence of their relative affinities with caloric, with oxygen,

at present in London, has favoured me with some particulars, in a letter, of which the following is an extract.

"Puis, par rapport au dollars, je serais bien surpris si jamais ils avaient été parfaitement purs, parce que, tant que je sais, ils ont toujours été frappés de cuivre de Fahlun, dont le minerais a toujours été plus ou moins melangé de plomb sulfuré, et peut-être d'antimoine. Cependant, comme ces dollars avaient été frappés originairement sous le regne du Roi FREDERIC, (FREDERICUS Rex Sueciæ,) et que dans ce tems là on aurait pu employer un minerais plus pur a Fahlun, il est probable que ces dollars, dans leur origine, ont été meilleurs que ceux contrefaits depuis; car ces dollars ayant été recherchés aux Indes, et surtout commandés en assez grande quantité par la Compagnie Asiatique de Copenhague, on en a frappé de nouveau, à plusieurs reprises, et de toute sortes de cuivre indifféremment. Ce fait explique au moins la cause de leurs inegalités de composition.

"Reste à savoir, si jamais on en a trouvé de parfaitement purs. Pour moi, je suis plus porté à croire, que c'est la convenance de la forme de ce cuivre qui l'a mis au courant dans les Indes, et que ce sont les prejugés qui lui ont donné du credit pour l'usage de la monnoie. Depuis quelques années, les Danois, ayant trouvé ce c[illegible] trop cher, en ont contrefait la marque eux mêmes, et en ont frappés a Ro[illegible] en Norwege; ce qui doit [illegible] encore une variété de ce cuivre."

or with both; and that these affinities become modified, by those which prevail between the various metallic substances and gold. Moreover, it is evident, that even the most oxidable metals have this property much diminished or checked by being united with gold, which appears so to envelope and retain their particles, as to impede the usual influence of heat, as well as the natural exertion of their affinities with the oxygen of the atmosphere. The following experiment was therefore made, to ascertain the comparative loss caused by the volatilization, or by the oxidizement of various metallic substances, when added to gold during a given period of fusion, and under similar circumstances.

Experiment.

Ten four-inch crucibles, which had been previously made red-hot, were put into as many 12-inch crucibles, which were placed in wind furnaces of similar construction, and heated as equally as possible. Each of the small crucibles contained five ounces ten pennyweights and fourteen grains of gold, 23 car. $3\frac{3}{4}$ grs. fine, which being completely melted, nine pennyweights and ten grains of the following metals were added, and mixed in the usual manner, after which, the fusion was continued in the open vessels during one hour.

The different masses, when cold, were weighed; but, previous to [illegible] or glass which had been formed on some of them was [illegible]. The comparative loss will appear from the following Table.

TABLE.

Weight of the Gold, and of the Alloy, before fusion.				Total Weight.	Weight of the Mass, after fusion.	Loss upon 6 oz.		
	car.	grs.		Ounces.	oz.	oz.	dts.	grs.
1. Gold,	23	$3\frac{3}{4}$ fine		6	6	0	0	0
	oz.	dts.	grs.					
2. Gold	5	10	14	6	6	0	0	0
Fine silver	0	9	10					
3. Gold	5	10	14	6	6	0	0	0
Copper	0	9	10					
4. Gold	5	10	14	6	6	0	0	0
Tin	0	9	10					
					oz. dts. grs			
5. Gold	5	10	14	6	5 19 21	0	0	3
Lead	0	9	10					
6. Gold	5	10	14	6	5 19 12	0	0	12
Fine iron	0	9	10					
7. Gold	5	10	14	6	5 19 12	0	0	12
Bismuth	0	9	10					
8. Gold	5	10	14	6	5 19 12	0	0	12
Antimony	0	9	10					
9. Gold	5	10	14	6	5 19 0	0	1	0
Zinc	0	9	10					
10. Gold	5	10	14	6	5 10 12	0	9	12
Arsenic	0	9	10					

According to the foregoing Table, it appears, that fine gold, gold alloyed with silver, gold alloyed with copper, and gold alloyed with tin, did not suffer any loss during the experiment.

Moreover, that gold alloyed with lead only lost three grains, chiefly by vitrification.

That gold alloyed with iron lost 12 grains, which formed scoria.

That gold alloyed with bismuth also lost 12 grains, chiefly by vitrification.

That gold alloyed with antimony lost the same quantity, partly by volatilization, and partly by vitrification.

That gold alloyed with zinc lost one pennyweight, by volatilization. And,

That gold alloyed with arsenic, not only lost the whole quantity of alloy, but also two grains of the gold, which were carried off in consequence of the rapid volatilization of the arsenic.

LEWIS, (Phil. Comm. of Arts, p. 88,) however, asserts that "gold is more volatilized by antimony than by arsenic or zinc; "but to produce this effect the fire must be vehement, the "crucible shallow, and the air strongly impelled." These circumstances, according to their variations, must undoubtedly very much influence the results of such experiments; and therefore, although the reverse was found to take place in the [illegible] stated, it does not follow that certain changes [illegible] by different degrees of heat, by the [illegible] air more or less strong.

The [illegible] section tend to prove, [illegible] general practice and opinion) only two of the [illegible] for the alloy of [illegible], namely, silver and copper; [illegible] alter the colour, [illegible] diminish the [illegible]. In respect to the [illegible].

the different metallic substances which have been employed in the present experiments, appear to affect gold nearly in the following decreasing order.

1. Bismuth. }
2. Lead. } These are nearly equal in effect.
3. Antimony. }
4. Arsenic.
5. Zinc.
6. Cobalt.
7. Manganese.
8. Nickel.
9. Tin.
10. Iron.
11. Platina.*
12. Copper.
13. Silver.

Before I conclude this section, I must observe, that the subject of alloys has not in general been sufficiently investigated by chemists; and I therefore should have been glad to have repeated the preceding experiments intirely in close vessels, and with many other precautions; but, from various circumstances, I have been obliged, for the present, to content myself with what has been here related; and, although I have not been able to do all I could have wished, I yet flatter myself that these experiments will tend to remove certain prejudices and erroneous opinions, and that they will be found to be of some utility.

* Had the platina been quite pure, the compound metal would probably have possessed more ductility; I cannot therefore take upon me to assert positively, that the place here assigned to platina, is precisely that which it ought to occupy.

SECTION II.

ON THE SPECIFIC GRAVITY OF GOLD, WHEN ALLOYED BY VARIOUS METALS.

The many difficulties which attend the making of experiments intended to ascertain the specific gravity of bodies, with any tolerable degree of precision, are sufficiently known to every one who has had practical experience; and some of these difficulties have been ably pointed out, and avoided, by Sir GEORGE SHUCKBURGH EVELYN, Bart. F. R. S. in his valuable Paper, entitled An Account of some Endeavours to ascertain a standard of Weight and Measure. Phil. Trans. for 1798, p. 133. In fact, when we consider the inaccuracies of balances, and the effects produced by the different height of the column of water, and by the changes of temperature to which the water is exposed during the experiments, we have less reason to be surprised at the frequent variations in the results; and, in addition to these sources of error, if we consider the different texture of bodies, and the numberless interstices of them, often unequally arranged, and which cannot at all times be penetrated by water, we may rather wonder that so much precision has [illegible] attained. The last-mentioned obstacles to exactitude in [illegible] peculiarly to be observed, when the spe- [illegible] stones and ores, is to [illegible] being in general more [illegible] variations in internal [illegible] specific gravities of these, [illegible] be regarded as exact; and indeed, [illegible] experiments. I think it will

appear evident, that extreme precision and uniformity in the results, can seldom be attained or expected.

When metals are cast in a mould, they speedily become cold; and, according to the quantity and quality of the metal, the figure and position of the mould, and the greater or less rapidity of the cooling, metals may vary in texture, and in the relative proportion and arrangement of their interstices; and consequently the mass, in different parts, may be of unequal degrees of density. For, a metal of an uniform quality in other respects, generally becomes most dense in the bottom of the mould, especially when a long bar of heavy metal is cast in a vertical position.

Those metals which are very ductile, may, by hammering and rolling, be brought more nearly to a certain uniform density; for the number and capacity of the interstices, or air-bladders, in the interior of the mass, are thus more or less diminished; and, although the brittle metals, or semimetals, as they are improperly called, cannot be thus treated, yet, when reduced to powder, or into small fragments, they expose a large surface, and consequently the error produced by interstices or cavities is much reduced.*

But, neither hammering, rolling, nor pulverization, can be applied to those metallic substances, whether simple or mixed,

* The interstices and cavities here mentioned, are those only which are formed during melting and casting; for the natural grain and texture peculiar to each metal, cannot be changed by any of the methods employed to correct the irregularities which have been accidentally introduced; and it appears proper to remark, that without very great caution, a new source of error may arise from the reduction of brittle metals into small fragments, or powder, which may conceal and retain little bubbles of air, so obstinately adherent to the metallic particles, as to require great patience and perseverance before they [illegible] and completely disengaged.

which are neither sufficiently malleable to be rolled, nor sufficiently brittle to be reduced into powder; and this last difficulty most frequently occurs in mixed or alloyed metals.

It is well known, that the specific gravity of an alloyed metal is seldom that which, by calculating the respective specific gravities and proportions of the different metals, would be the result; on the contrary, the specific gravity of the alloyed mass, is frequently greater or less than it ought to be, according to calculation.

This effect has been often noticed by various authors; and it is not requisite that I should here repeat facts already so well established; I have, however, thought it proper to state, in the following pages, the changes in specific gravity which took place, when gold was alloyed with different proportions of various metals.

In the following experiments, I employed a very accurate balance, which was made for me by Mr. HAAS, and which, when loaded with 1000 grains at each end, turned with $\frac{1}{200}$ of a grain.

The vessel containing distilled water, at 60° of FAHRENHEIT, was covered with flannel, in order to avoid, as much as possible, any change of temperature produced by the circum[illegible] air; and every other precaution was taken, as is usual, [illegible] are to be made.*

[illegible] one Troy ounce of alloyed [illegible] different ingots and bars [illegible]

TABLE I.

Gold variously alloyed.	oz.	dts.	grs.	Specific Gravity.
Gold, 23 car. 3¾ grs. fine, which had been rolled and stamped - - -				- - 19,277.
Gold, 23 3½ fine, in the bar - -				- - 19,172.
Gold, 23 3½ fine - - -	0	18	10	- - 19,013.
Platina* - - -	0	1	14	
Gold, 23 3½ fine - - -	0	18	10	- - 16,816.*
Platina - - - -	0	1	14	
Copper - - -	0	1	14	
Gold, 23 3½ - -	0	18	10	- - 17,927.
Pure silver - - -	0	1	14	
Gold, 23 3½ - - -	0	18	10	- - 17,344.
Pure silver - - -	0	0	19	
Copper - - - -	0	0	19	
Gold, 23 3½ - - -	0	18	10	- - 17,157.†
Copper - - -	0	1	14	
Gold, 23 3½ - -	0	18	10	- - 16,885.‡
Pure wrought iron - -	0	1	14	

* The specific gravity of the platina was 18,717 ; and it has been already remarked, that it contained a small portion of iron.

† The experiments upon gold made standard by silver, by silver and copper, and by copper alone, were made upon whole and complete ingots, which had been mixed and cast with the greatest care, and which, separately, weighed two ounces Troy.

‡ If this metal had been cast in sand, it would have been porous, and the specific gravity would have been less than is stated in the table ; on this account, sand moulds were not employed in these experiments.

The mould of iron, which has been so frequently mentioned, was not a common open ingot mould, but was a box of polished wrought iron, with a lid, which was ground so as to be air-tight. The mouth was at one end ; and a bar cast in this mould measured 12 inches in [illegible] inch in breadth, and [illegible] of an inch in thickness.

Table I. *(continued.)*

Gold variously alloyed.	car.	grs.	oz.	dts.	grs.	Specific Gravity.
Gold,	23	3½	0	18	10	16,840.
Cast steel			0	1	14	
Gold,	23	3½	0	18	10	17,125.
Pure iron			0	0	19	
Copper			0	0	19	
Gold,	23	3½	0	18	10	17,307.
Tin			0	1	14	
Gold,	23	3½	0	18	10	17,278.
Tin			0	0	19	
Copper			0	0	19	
Gold,	23	3½	0	18	10	17,352.
Copper			0	1	6	
Tin			0	0	8	
Gold,	23	3½	0	18	10	18,080.
Lead			0	1	14	
Gold,	23	3½	0	18	10	17,765.
Lead			0	0	19	
Copper			0	0	19	
[illegible]	[illegible]	[illegible]	0	18	10	17,312.
[illegible]			0	1	6	
[illegible]			0	0	8	
[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]	17,082.
[illegible]			[illegible]	[illegible]	19	
[illegible]			0	[illegible]	[illegible]	
Gold,	[illegible]	[illegible]	0	[illegible]	[illegible]	16,627.
Copper			[illegible]	[illegible]	[illegible]	

TABLE I. *(continued.)*

Gold variously alloyed.	car.	grs.	oz.	dts.	grs.	Specific Gravity.
Gold,	23	$3\frac{1}{2}$	0	18	10	17,039.
Copper			0	1	$13\frac{3}{4}$	
Lead			0	0	$0\frac{1}{4}$	
Gold,	23	$3\frac{1}{2}$	0	18	10	18,038.
Bismuth			0	1	14	
Gold,	23	$3\frac{1}{2}$	0	18	10	17,302.
Copper			0	1	6	
Bismuth			0	0	8	
Gold,	23	$3\frac{1}{2}$	0	18	10	16,846.
Copper			0	1	10	
Bismuth			0	0	4	
Gold,	23	$3\frac{1}{2}$	0	18	10	16,780.
Copper			0	1	$13\frac{1}{2}$	
Bismuth			0	0	$0\frac{1}{2}$	
Gold,	23	$3\frac{1}{2}$	0	18	10	17,095.
Copper			0	1	$13\frac{3}{4}$	
Bismuth			0	0	$0\frac{1}{4}$	
Gold,	23	$3\frac{1}{2}$	0	18	10	16,937.
Zinc			0	1	14	
Gold,	23	$3\frac{1}{2}$	0	18	10	17,175.
Zinc			0	0	19	
Copper			0	0	19	
Gold,	23	$3\frac{1}{2}$	0	18	10	17,402.*
Copper			0	1	6	
Zinc			0	0	8	

* The chief part of the zinc appeared to have been volatilized.

TABLE I. *(continued.)*

Gold variously alloyed.	car. grs.	oz.	dts.	grs.	Specific Gravity.
Gold,	23 3½	0	18	10	17,293.
Fine brass		0	1	14	
Gold,	23 3½	0	18	10	16,914.
Brass		0	0	19	
Copper		0	0	19	
Gold,	23 3½	0	18	10	17,112.
Cobalt		0	1	14	
Gold,	23 3½	0	18	10	17,255.
Cobalt		0	0	19	
Copper		0	0	19	
Gold,	23 3½	0	18	10	17,341.
Copper		0	1	6	
Cobalt		0	0	8	
Gold,	23 3½	0	18	10	17,286.
Copper		0	1	10	
Cobalt		0	0	4	
Gold,	23 3½	0	18	10	17,068.
Nickel		0	1	14	
Gold,	23 3½	0	18	10	17,298.
[illegible]		0	0	19	
[illegible]		0	0	19	
Gold,	23 3½	0	18	10	17,382.
[illegible]		0	[illegible]	[illegible]	
[illegible]		0	0	[illegible]	
Gold,	[illegible]	0	[illegible]	10	17,260.
Copper		0	[illegible]	10	
[illegible]		0	0	4	

TABLE I. *(continued.)*

Gold variously alloyed.	car	grs.	oz.	dis.	grs.	Specific Gravity.
Gold,	23	3½	0	18	10	16,929.
Antimony			0	1	14	
Gold,	23	3½	0	18	10	17,147.
Antimony			0	0	19	
Copper			0	0	19	
Gold,	23	3½	0	18	10	17,258.
Copper			0	1	6	
Antimony			0	0	8	
Gold,	23	3½	0	18	10	17,169.*
Copper			0	1	10	
Antimony			0	0	4	
Gold,	23	3½	0	18	10	17,073.
Copper			0	1	13¾	
Antimony			0	0	0¼	

The preceding experiments afford some remarkable results, produced in the specific gravity of gold, by the addition of certain metallic substances in various proportions; but, as these results are sufficiently pointed out in the subsequent pages, it would be superfluous to enter into a recapitulation of the whole.

The effects, however, produced upon gold by lead and bismuth, are peculiarly worthy of notice, not only on account of the alterations in specific gravity, but also from the remarkable similarity of the effects of these two metals, when employed as alloys in proportions relatively equal. This will appear more evident by the following comparative statement.

* [illegible] the antimony were lost by meltin[illegible]

Gold alloyed with lead.*	dts.	grs.	Spec. Grav.	Gold alloyed with bismuth.†	dts.	grs.	Spec. Grav.
Gold -	18	10	18,080.	Gold	18	10	18,038.
Lead -	1	14		Bismuth	1	14	
Gold	18	10	17,765.	Gold	18	10	not examined.
Copper	0	19		Copper	0	19	
Lead -	0	19		Bismuth	0	19	
Gold -	18	10	17,312.	Gold	18	10	17,303.
Copper	1	6		Copper	1	6	
Lead -	0	8		Bismuth	0	8	
Gold	18	10	17,032.	Gold	18	10	16,846.
Copper	1	10		Copper	1	10	
Lead -	0	4		Bismuth	0	4	
Gold	18	10	16,627.	Gold	18	10	16,780.
Copper	1	13½		Copper	1	13½	
Lead -	0	0½		Bismuth	0	0½	
Gold	18	10	17,039.	Gold	18	10	17,095.
Copper	1	13¾		Copper	1	13¾	
Lead -	0	0¼		Bismuth	0	0¼	

* Specific gravity of the lead 11,352.

† Specific gravity of the bismuth 9,822.

Although the specific gravities of lead and of bismuth are so different, yet the effects which these metals produce upon the [illegible] of gold, according to their relative proportions, [illegible] to every one who examines the fore-[illegible] the quality of the gold thus made [illegible] by different proportions of lead and bismuth, [illegible] with the alterations of specific gravity; for, in the first [illegible] those in which lead in the [illegible] case and bismuth in the other formed the whole of the alloy, the gold [illegible] and showed a fine [illegible]

fracture, which had a porcellaneous appearance; but, in the subsequent experiments, in proportion as the specific gravity was reduced, the grain of the fracture became coarser; and, in the IVth *Experiment*, the porcellaneous or earthy appearance of the fracture began to give place to a certain degree of metallic lustre, which was increased in the Vth *Experiment;* at the same time, the alloyed gold, in both cases, became remarkably coarse-grained and spongy.

The specific gravity was then found to be at the lowest degree; for, in the VIth or last *Experiment*, when only $\frac{1}{4}$ of a grain of lead, or of bismuth, was present, the grain became compact, with complete metallic lustre; and the specific gravity was so much increased, that when lead was employed, the difference between the Vth and VIth *Experiments* was 0,412; and, when bismuth was present, the difference was 0,315.

From these and other experiments, I am induced to believe, that, in general, the specific gravity of gold alloyed with different metals, is not only very different to what it ought to be according to calculations made on the relative proportions and specific gravity of the alloy, but that it is also subject to many variations, partly occasioned by peculiar effects produced by certain proportions of some of the metals, and partly by effects peculiar to certain compound alloys; so that, by the proportions of certain metals, and by the combination of these with others, an immense complicated series of alterations in specific gravity are produced, which as yet do not appear to have been investigated, by those philosophers who have written concerning the specific gravity of metals.

The specific gravity of standard gold being found by the preceding experiments to be so extremely variable, according to the nature and quantity of the metals which were employed singly or conjointly as alloys, the following Table has been added, to show the comparative degrees of expansion and contraction which took place, in consequence of these combinations.

TABLE II.

Metals.	Specific gravity.	Weight.	Bulk before combination, in grains of water.		Bulk after combination.	Expansion.	Contraction.	Specific gravity of the mass.
		grains.						
Gold - Silver -	19,172 10,474	442 38	23,05 3,63	26,68	26,78	,10	—	17,927
Gold - Silver - Copper -	19,172 10,474 8,895	442 19 19	23,05 1,81 2,14	27,00	27,67	,67	—	17,344
Gold - Copper -	19,172 8,895	442 38	23,05 4,27	27,32	27,98	,66	—	17,157
Gold - Iron -	19,172 7,700	442 38	23,05 4,94	27,99	28,43	,44	—	16,885
Gold - Iron - Copper -	19,172 7,700 8,895	442 19 19	23,05 2,47 2,14	27,66	28,03	,37	—	17,125
Gold Tin	19,172 [illegible]	442 38	23,05 5,21	28,26	27,73	—	,53	17,307
Gold Tin Copper -	[illegible] [illegible] [illegible]	[illegible] [illegible] 19	[illegible] [illegible] 2,14	[illegible]	27,78	—	,02	17,278
Gold - [illegible] Tin -	19,172 [illegible] [illegible]	[illegible] [illegible] 2	23,05 3,37 1,10	27,52	27,66	,14	—	17,352
Gold - [illegible] -	19,172 [illegible]	[illegible] [illegible]	[illegible] [illegible]	26,40	26,54	,14	—	18,080

TABLE II. *(continued.)*

Metals	Specific gravity.	Weight.*	Bulk before combination, in grains of water.		Bulk after combination.	Expansion.	Contraction.	Specific gravity of the mass.
		grains.						
Gold -	19,172	442	23,05	26,86	27,02	,16	—	17,765
Lead -	11,352	19	1,67					
Copper -	8,895	19	2,14					
Gold -	19,172	442	23,05	27,12	27,73	,61	—	17,312
Copper -	8,895	30	3 37					
Lead -	11,352	8	,70					
Gold -	19,172	442	23,05	27,22	28,18	,96	—	17,032
Copper -	8,895	34	3,82					
Lead -	11,352	4	,35					
Gold -	19,172	442	23,05	27,31	28,87	1,56	—	16,627
Copper -	8,895	37,50	4,22					
Lead -	11,352	0,50	,04					
Gold -	19,172	442	23,05	27,31	28,17	0,86	—	17,039
Copper -	8,895	37,75	4,24					
Lead -	11,352	0,25	,02					
Gold -	19,172	442	23,05	26,92	26,61	—	,31	18,038
Bismuth	9,822	38	3,87					
Gold -	19,172	442	23,05	27,24	27,74	,50	—	17,302
Copper -	8,895	30	3,37					
Bismuth	9,822	8	,82					
Gold -	19,172	442	23,05	27,28	28,49	1,21	—	16,846
Copper -	8,895	34	3,82					
Bismuth -	9,822	4	,41					
Gold -	19,172	442	23,05	27,32	28,61	1,29	—	16,780
Copper -	8,895	37,50	4,22					
Bismuth -	9,822	0,50	,05					
Gold -	19,172	442	23,05	27,32	28,07	,75	—	17,095
Copper -	8,895	37,75	4,24					
Bismuth -	9,822	0,25	,03					
Gold -	19,172	442	23,05	28,43	28,34	—	,09	16,937
Zinc -	7,065	38	5,38					
Gold -	19,172	442	23,05	27,88	27,95	,07	—	[illegible]
Zinc -	7,065	19	2,69					
Copper -	8,895	19	2,14					

TABLE II. *(continued.)*

Metals.	Specific gravity.	Weight.	Bulk before combination, in grains of water.		Bulk after combination.	Expansion.	Contraction.	Specific gravity of the mass.
		grains						
Gold - Copper - Zinc -	19,172 8,895 7,065	442 30 8	23,05 3,37 1,13	27,55	27,58	,03	—	17,402
Gold - Cobalt -	19,172 7,645	442 38	23,05 4,97	28,02	28,05	,03	—	17,112
Gold - Cobalt - Copper -	19,172 7,645 8,895	442 19 19	23,05 2,48 2,14	27,67	27,82	,15	—	17,255
Gold - Copper - Cobalt -	19,172 8,895 7,645	442 30 8	23,05 3,37 1,05	27,47	27,68	,21	—	17,341
Gold - Copper - Cobalt -	19,172 8,895 7,645	442 34 4	23,05 3,82 ,52	27,39	27,77	,38	—	17,286
Gold - Nickel -	19,172 7,807	442 38	23,05 4,87	27,92	28,12	,20	—	17,068
Gold - Nickel - Copper -	19,172 7,807 8,895	442 19 19	23,05 2,43 2,14	27,62	27,75	,13	—	17,298
Gold - Copper - Nickel -	19,172 8,895 7,807	442 30 8	23,05 3,37 1,03	27,45	27,61	,16	—	17,382
Gold - Copper - Nickel -	19,172 8,895 7,807	442 34 4	23,05 3,82 ,51	27,38	27,83	,45	—	17,250
Gold - Antimony	19,172 6,712	442 38	23,05 5,66	28,71	28,35	—	,36	16,929
Gold - Antimony Copper -	19,172 6,712 8,895	442 19 19	23,05 2,83 2,14	28,02	27,99	—	,03	17,147
Gold - Copper - Antimony	19,172 8,895 6,712	442 30 8	23,05 3,37 1,19	27,61	[illegible]	[illegible]	—	17,258

TABLE II. *(continued.)*

Metals.	Specific gravity.	Weight	Bulk before combination, in grains of water.		Bulk after combination.	Expansion	Contraction.	Specific gravity of the mass.
		grains.						
Gold -	19,172	442	23,05					
Copper -	8,895	34	3,82	27,47	27,94	,47	—	17,169
Antimony	6,712	4	,60					
Gold -	19,172	442	23,05					
Copper -	8,895	37,75	4,24	27,33	28,11	,78	—	17,073
Antimony	6,712	0,25	,04					

Although the experiments upon which I have formed the preceding Table were made with considerable care and attention, yet it certainly would not be right to suppose the degrees of expansion or contraction to be rigidly and exactly determined in every fractional part; for, besides the almost impossibility of totally preventing the escape of some part of the more volatile metals, even a variation in the degree of heat during melting, as well as in the mode of cooling, must make some difference, for which an allowance ought to be made; but these unavoidable inaccuracies, do not prevent the more general and essential effects from being ascertained.

Very little alteration appears to have been produced by alloying gold with $\frac{1}{12}$ of pure silver;* for the alloyed mass only differed from the natural bulk of the two metals by ,10; and this accords with former observations upon the effects which these metals produce on each other.

But, in the next case, which consisted of gold alloyed with

* In this and every other case, when the proportion of alloy was estimated, an allowance has been made for the deficiency in the quality of the gold, amounting to half a carat grain.

equal parts of silver and copper, the expansion amounted to ,67; which is the more remarkable, as, in the subsequent article, copper being employed singly, produced only an expansion of ,66. It appears, therefore, that the compound alloy of silver and copper, being added in the proportion of $\frac{1}{12}$ to gold, causes a degree of expansion superior to that produced by copper, although it might be previously imagined, that the silver would have checked or diminished the expansive property of the copper.

$\frac{1}{12}$ of iron appears to have caused an expansion rather inferior to that of copper; but an alloy composed of equal parts of iron and copper, produced an expansion less than the former. This effect seems also to be peculiar to this compound alloy; for, according to the effects which copper was found singly to produce upon gold, the compound alloy of iron and copper ought to have produced an expansion superior to that caused by iron alone.

A considerable contraction was caused when $\frac{1}{12}$ of tin was added to gold; but, as an attempt had been made to pass the mass between rollers, before the specific gravity was taken, the contraction must not be estimated at so much as ,53.

When gold was alloyed with equal parts of tin and copper, the contraction was found to be only ,02; but, in the next case, when the copper amounted to 30 grains, and the tin only to 8, an expansion took place, equal to ,14.

$\frac{1}{12}$ of lead produced an expansion equal to ,14; but, from the similarity of all the other effects of lead to those of bismuth, I am inclined to believe that lead, in some proportion greater than $\frac{1}{12}$, would produce contraction. In all the instances, however, stated in the Table, expansion was observed; and, when lead was in the proportion of 4 grains to 34 of copper, or of

half a grain to $37\frac{1}{2}$ of the same metal, then a very remarkable expansion took place, which seemed to be a peculiar effect of this compound alloy; for, in the subsequent case, when the lead was reduced to $\frac{1}{4}$ of a grain, the degree of expansion was much less.

Bismuth, in its various properties, as I have several times had occasion to observe, very much resembles lead, in respect to the effects which it produces upon gold, excepting, that when employed singly, and in the proportion of $\frac{1}{12}$, it occasioned a contraction equal to ,31. But, in smaller quantities, and in conjunction with copper, it produced expansion, which became very considerable, when bismuth was added in the proportion of 4 grains, or of half a grain, per ounce; so that what has been already said concerning lead may here be repeated.

$\frac{1}{12}$ of zinc caused the mass to contract ,09; but the volatility of this metal renders the results very uncertain. In the last article, nearly the whole of the 8 grains of zinc were volatilized.

It is not necessary to make any remarks on the effects produced by cobalt and nickel upon gold; and, in respect to antimony, we may observe, that contraction was produced in the two first cases, but expansion in all the others. Indeed, from a general view of the Table it appears, that those metals which most readily render gold brittle, are those which have the greatest tendency to produce contraction, when added to gold in certain proportions.

In some cases, the degree of expansion seems to increase with the proportion of copper; but then it must be observed, that this increase of expansion is frequently much more considerable

than that which ought to be produced, supposing this effect depended only upon the quantity of copper; it may therefore be inferred, that the properties of a compound metal are peculiar to itself, and are in general different from the mean of the properties of the several metals employed to form the compound.

The results stated in the foregoing Table, seem to indicate, that the assertions of many respectable authors, concerning the density of alloyed metals, should not be understood in an absolute or unqualified sense.

Mr. BRISSON, in his valuable work entitled *Pèsanteur specifique des Corps,* has observed, that a mutual penetration takes place, when eleven parts of gold are alloyed with one of copper; and, in consequence of this, that he found the specific gravity of gold alloyed with $\frac{1}{12}$ of copper to be 17,486; although, if this mutual penetration of the two metals had not happened, the specific gravity ought to have been 17,153; but, in the course of the present experiments, the reverse of this has been observed; for, instead of any mutual penetration of these metals, a very notable degree of expansion in the alloyed mass has been remarked.

When 442 grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, (the specific gravity of which was 19,172,) were alloyed with 38 grains of fine copper, of the specific gravity of 8,895, the mass was found to be of a specific gravity equal to 17,157; and, as the bulk of this alloyed mass amounted to 27,98, while the natural bulk of the two metals before combination amounted only to 27,32, there was consequently an expansion of the alloyed mass, equal to ,66. These calculations were made upon an intire ingot, weighing two ounces Troy, or 960 grains; which mode appeared

to me to be more accurate, than if the experiments had been made upon part of a large mass or ingot. In the latter case there are many sources of error, which either have been or will be noticed in the present Paper; and the observations made upon them apply to the subject under immediate examination, as well as to the specific gravity of compound or alloyed metals in general.

If Mr. BRISSON made his experiment upon part of a large bar or ingot, (which probably was the case,) it will not be difficult to conceive the reason why he found the specific gravity to be 17,486. For, the unequal diffusion of the alloy, the quantity of the metal, with the *nature*, *form*, and *position*, of the mould, will always produce variations in specific gravity.

In some experiments, when copper was present in rather a less proportion than the above, still a very conspicuous degree of expansion prevailed, even in that part of the mass which was subjected to the pressure of a considerable quantity of superincumbent metal, and even when the whole was cast in a mould of iron, which, from repeated experiments, I have found to be unfavourable to the expansion of metals. As a proof of this, I shall state an experiment which will again be found in a subsequent part of this Paper, but which may here be anticipated with propriety, as it tends to elucidate the present subject.

Experiment.

A quantity of gold, 23 car. $3\frac{1}{2}$ grs. fine, was alloyed with fine Swedish copper, in such a proportion as to form an uniform mass, which, by assays made upon both extremities, proved to be 8 Troy grains in the pound better than standard.

Two pounds of this alloyed gold were cast in a mould of

-on, by which a bar was formed, nearly 12 inches in length, one inch in breadth, and one quarter of an inch in thickness. Every possible precaution had been taken, to mix and diffuse the copper uniformly throughout the gold; and the assays which were made subsequent to the casting, fully proved that the mixture was perfect. When, however, the specific gravity of the two extremities, or of the top and bottom ends of the bar, was examined, it appeared, that the specific gravity of the upper end was 17,035, while that of the lower end was 17,364. So that, although the quality of the bar was perfectly equal in every part, yet, by the pressure of the superincumbent metal, the lower extremity, or that which was formed in the bottom of the mould, had acquired a very superior degree of density.

Now, from the foregoing Table it appears, that the bulk of 442 grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, is very nearly 23,05, and that the bulk of the fine Swedish copper employed in these experiments is, for 38 grains, equal to 4,27; consequently, the total bulk of these two metals, before combination, amounts to 27,32. Moreover, when these metals were combined, then the bulk of 480 grains of this standard gold was 27,98; so that an expansion equal to ,66 had taken place, in conse-[illegible] of the combination of 442 grains of the gold with 38 [illegible]. In order to compare this result with the bar [illegible] be first remembered, that the quality of [illegible] better than standard; [illegible] of 442,66 [illegible] of the [illegible] of the 17,34 grs. [illegible] 27,27 [illegible] total bulk [illegible]

this had been effected, and the bar had been cast as already mentioned, then the bulk of the upper extremity was found to be 28,18, while that of the lower end was only 27,64. The difference, therefore, between 27,27 and 28,18, shows, that an expansion equal to ,91 had taken place in the upper end of the bar; and the difference between 27,27 and 27,64, also shews, that an expansion equal only to ,37 had taken place in the inferior extremity; and that thus a difference of ,54, in the expansion of the two extremities, had been produced merely by the pressure of the superincumbent metal. Had it not been for this circumstance, there is reason to believe, that the general expansion of the whole mass would have been nearly the same as that of the standard gold mentioned in the Table, namely, ,66; for the expansion of the upper end of the bar being ,91, and that of the lower end being ,37, the mean consequently must be ,64, which (taking into consideration the small difference in the quality of the two kinds of alloyed gold) may be regarded as a very near approximation to the statement in the Table.

It will now be proper to notice other causes, which more or less influence the specific gravity of what is called standard gold.

The most frequent cause of variations, in the specific gravity of gold made standard by silver or copper, is the unequal diffusion of the alloy throughout the mass of gold; for an exact distribution of the alloy is not so easily made as may be imagined, especially when a large quantity of gold is to be alloyed.

In Mints, this difficulty has however been considered, and an allowance has been made for it, which is called the Remedy for the Master of the Mint.

According to this regulation, when the trial of the Pix, as it

is called, takes place before the Lord Chancellor, the Lords Commissioners of the Treasury, &c. &c. &c. the Master of the Mint is held excusable, if the imperfection or deficiency of the coin, in the aggregate, is less than the sixth part of a carat, equal to 40 grains of fine gold, in the pound of standard, or the 132d part of the value.

When it is considered, that the extreme accuracy of philosophical experiments cannot easily be introduced into such establishments as mints, where the work is carried on upon a large scale, some latitude may with reason be expected, and granted, especially as a perfectly exact mixture of the alloy is attended with difficulty.

In Hellot's French translation of Schlutter's work, entitled *Essais des Mines et des Metaux*, p. 276, the following experiment is mentioned, in order to prove the frequent unequal mixture of gold with another metal, such as silver.

"A quantity of silver, amounting to upwards of twenty "pounds, and containing about a 56th part of gold, was melted "in a crucible, and poured into cold water, in order that it might "be granulated: by dipping, at different times, an iron ladle "into the water under the stream of metal, parts of the first, "second, and third running were separately received; and, "being assayed, were all found to differ in their content of gold."

[illegible] noticed this experiment, also describes another, made by Mr. Homberg, which is related in the Memoirs of the Academy of Paris, for the year 1713.

"Equal parts of gold and silver, melted together, and reduced "into fine grains, were put into a crucible, with a mixture of "about equal parts of decrepitated sea salt and rough nitre "under them: the crucible being kept in a small fire in a wind

"furnace for about a quarter of an hour, and then suffered to "cool, was broken; the gold was then found in one lump at "the bottom, and the silver above it, in two pieces, with some "grains enveloped in the salts, which had not been intirely "melted. The silver was perfectly pure, and without the least "mixture of gold; but the gold retained about one-sixth part "of silver."

He repeated the experiment, with different mixtures of the two metals, and found the silver to be always free from gold, but that the gold retained a little of the silver, except in two instances, in which this was also pure. Mr. HOMBERG observes, that "unless the gold and silver are nearly in equal "quantities, the separation does not succeed; and that the only "nicety in the process consists in hitting the due point of fusion; "for, if the fire is too long continued, or the mixt made to flow "thin, the two metals, after they have parted from one another, "mingle again together." LEWIS's Phil. Comm. of Arts, p. 86.

From these experiments it appears, that the equal distribution and mixture of two metals, such as gold and silver, is by no means very easy to be made, without certain precautions; and also, that when they have been completely mixed, if they are kept in fusion under certain circumstances, a separation, more or less perfect, sometimes takes place: This separation appears to be according to the relative affinities and specific gravities of the two metals, and is the soonest effected when the metals have not been perfectly mixed.*

Soon after the commencement of the experiments at the Mint,

* Some compound metals may perhaps be mere mechanical mixtures; but I am inclined to believe, that by much the greatest number are true chemical combinations; and consequently, when these last have been properly formed, a separation of the component metals, by the means abovementioned, can seldom if ever be effected.

I was desirous to examine the specific gravity of some bars of gold, which had been made standard by the addition of various kinds of copper; and, as every usual precaution had been taken to mix the alloy properly with the gold, the pieces which were to be hydrostatically weighed were taken from the ends of the bars, without any discrimination whether the pieces were cut from the end of the bars which, when cast, had been formed near the mouth of the mould, or from that end which had been formed at the bottom. The following were the results.

		Specific gravity.
1.	Gold made standard by the best Swedish copper, the bar being cast in a mould of iron -	17,372
2.	The same bar melted again, and cast in sand -	17,312
3.	Gold made standard by common Swedish copper, cast in sand - - - -	16,225
4.	Gold made standard by Swedish dollar copper, cast in sand - - - - -	16,977
5.	Gold made standard by British copper, cast in iron	17,281
6.	The same, cast in sand - - -	16,994
7.	Gold made standard with another sample of British copper, cast in sand - -	16,979

From these experiments it was evident, that when the same metal was cast in iron and in sand, a difference was to be observed in the specific gravity, which was always the most considerable when moulds of iron were employed; but, allowing that this might have operated at certain times, yet so great a variation was discovered in other instances, that it was thought requisite to make a new series of experiments, in order to ascertain the cause; and, as there was reason to suspect that part, at least, of this difference in specific gravity arose from individual

distribution of the alloy, two pieces were taken from the opposite extremities of each bar, and were examined as follows.

	Specific gravity.	
	Upper end.	Lower end.
1. Gold made standard by silver -	18,273	17,186
2. Gold made standard by equal parts of silver and copper - - -	18,062	16,659
3. Gold made standard by copper -	18,492	16,680
4. Gold made standard by lead -	18,124	18,037
5. Gold made standard by equal parts of copper and iron - - -	17,068	16,924
6. Gold made standard by an alloy composed of $\frac{3}{4}$ copper and $\frac{1}{4}$ of tin -	17,551	16,747
7. Gold made standard by antimony -	17,121	16,707

Each of the bars weighed two pounds Troy; they were one inch broad, and $\frac{1}{4}$ of an inch in thickness; and were cast in a mould of iron.

From these experiments it appeared, that the upper extremities of these bars, or those which had been formed at or near the mouth of the mould, were uniformly of greater specific gravity than the opposite ends of the same bars, or those which had been formed in the bottom of the mould; and that the smallest variation in the specific gravity of the two ends of a bar, was in that which consisted of gold alloyed with lead.

The above mixtures were made with the usual precautions, such as rapid stirring, and pouring; but, nevertheless, it seemed that the alloy had never been completely and uniformly distributed throughout the mass of gold; or, if it really had been well diffused and mixed, that it subsequently (although in a very short time) had again separated, according to its relative

specific gravity to that of gold. Therefore, as the upper end of each bar was uniformly of a much greater specific gravity than the opposite extremity, and as the metal was speedily congealed in the mould, and as the contents of each crucible, when poured, occupied the mould in an inverted order, (the metal at the bottom of the crucible being that which was last poured, and consequently being that which formed the upper extremity of the bar,) there was much reason to believe that the alloy was not equally distributed, and that the melted mass, when in the crucible, varied in quality, so that the lower part consisted of gold above standard, and the upper part, of gold inferior to standard; and, as but little alteration could take place when the metal was poured, this unequal quality remained, although inverted in respect to situation.

In consequence of these experiments, it became necessary to contrast them with comparative assays.

Several of these were therefore made; but I shall only mention such as are immediately requisite to determine the question. It is proper, however, to remark, that the upper extremities of the bars which have been mentioned, were all found to be better than standard, while the inferior extremities proved to be worse. But the experiments to which I immediately allude are the following.

Experiment 1.

Twenty-two ounces two pennyweights and four grains of gold, 23 car. 3½ grs. fine, were alloyed with one ounce seventeen pennyweights and eighteen grains of copper. When the whole was completely melted, it was rapidly stirred, and was then suffered to continue in fusion during half an hour; after which, it was poured into a mould of iron.

The rough end of the bar, next the mouth of the mould, was cut off, and then three pieces were taken, viz. one from the upper extremity, another from the middle, and a third from the bottom.

The specific gravity of these three pieces was ascertained; and they were then assayed by Mr. BINGLEY, his Majesty's Assay Master at the Mint, who reported them as follows.

	Specific gravity.	Quality by assay.
1. Top -	18,141	Better than standard, $3\frac{1}{2}$ carat grs.* = 210 Troy grains.
2. Middle -	17,043	Worse than standard, $1\frac{3}{4}$ carat grs. = 105 Troy grains.
3. Bottom	16,689	Worse than standard, $3\frac{3}{4}$ carat grs. = 225 Troy grains.

This experiment therefore proved, that as the upper extremity, or No. 1, was superior in specific gravity to the middle of the bar, or No. 2, so this last was superior to the bottom, or lower extremity, No. 3; in like manner, the quality of the gold was much better in No. 1 than in No. 2; and this also considerably surpassed the quality of the last, or No. 3.

But it was still uncertain, supposing the alloy to have been perfectly mixed, whether it would readily become partly separated from the gold, so as to leave the mass thus of different qualities.

To determine this, the next experiment was made.

* One carat grain is equal to 60 Troy grains.

Experiment II.

Eleven ounces one pennyweight and two grains of gold, 23 car. $3\frac{1}{2}$ grs. fine, were alloyed with nineteen pennyweights and six grains of copper; the whole being well melted, was stirred with a large earthen stirrer; and, in order the better to mix the alloy with the gold, the melted metal was poured alternately into two red-hot crucibles, after which, it was cast in the mould of iron. The rough end of the bar was cut off, and a piece was then taken from each extremity.

The specific gravity of the upper end was - 17,035
And that of the lower end was - - 17,364.

So that, according to this experiment, the bottom end of the bar possessed the greatest specific gravity, contrary to the results of the former experiments.

By an assay of each piece, made by Mr. BINGLEY, it however appeared, that this difference in specific gravity was not caused by any unequal quality of the gold, for the proportion of alloy in the two pieces was found to be precisely the same; the alloy had therefore been uniformly mixed.

	Specific gravity.	Quality by assay.
1. Top	17,035	Better than standard, by 8 grains Troy.
2. Bottom	17,364	Better than standard, by 8 grains Troy.

It now remained, therefore, to examine whether the copper alloy, which was thus regularly distributed throughout the mass, could again be induced to separate by a subsequent fusion.

Experiment III.

The bar which had been made in the preceding experiment, was again melted, and was kept in complete fusion during half

an hour, without being stirred or agitated; it was then cast as before.

The two extremities, being separated, afforded the following results.

	Spec. gravity.	Quality by assay.
1. Top	17,203	Better than standard, by 10 grains Troy.
2. Bottom	17,387	Better than standard, by 10 grains Troy.

This last bar was, therefore, throughout of an equal quality; although it appeared that, by this second fusion, the whole mass was become finer, by two Troy grains, than it was in the former experiment; and the specific gravity of both ends was also become more considerable.

In these two last experiments, the specific gravity of the bars was the greatest at the bottom; and as by the assays it was proved, that each bar was of an uniform quality, it may be inferred, that when a bar of metal is cast in, or nearly in, a vertical position, a difference in the density of the mass takes place, independent of any change in the quality of the mixture, and that the greatest density prevails in the lower part of the column, or in that which suffers the greatest pressure from the superincumbent metal. It also follows, that this effect is subject to be modified by the quality and specific gravity of the metal, by the more or less vertical position of the mould, by the quantity of metal which is cast, and especially by the length of the bar, or height of the metallic column.

There cannot be any doubt but that the same causes operated in those experiments which afforded results so precisely opposite to these last, in respect to the relative specific gravities of the extremities of the bars; but then, the effect in question was much more than compensated by the unequal distribution of

the alloy, which predominated in the upper part of the mass when in the crucible, and consequently was the first which entered and filled the lower part of the mould, so that the finer and more heavy gold, at the bottom of the crucible, was that which formed the upper part of the bar; and it must be obvious, that the congelation was too rapid in the mould, to allow any very material change to take place after the metal was poured.

The foregoing facts being considered, it is possible to conceive, that a bar of alloyed gold may be throughout of equal specific gravity, and nevertheless not be of an uniform value or quality; for the finer quality of the upper extremity, when not considerable, may at times be compensated by the superior density of the bottom; but such effects can only take place within a very limited sphere.

Exclusive of the causes lately enumerated, which occasion variations more or less considerable in the specific gravity of metals, there is another, which, I believe, has never been noticed; it is true that its effects, when compared with those already mentioned, are but small; but still it appears proper that it should be taken into consideration, in the course of the present investigation. Long continued friction, is the cause to which I now allude; for I have always found, that it produced a diminution in the specific gravity of those pieces of metal which had been subjected to it, as the following experiments will prove.

Experiment 1.

In this experiment, forty-two pieces of gold, differently alloyed, and of the diameter of a guinea, were taken in the following order.

1. Gold of 23 car. $3\frac{3}{4}$ grs. fine - - - 6 pieces.
2. Gold alloyed with silver - - - 6 pieces.
3. Gold alloyed with silver and copper - 6 pieces.
4. Gold alloyed with copper - - - 6 pieces.
5. Gold alloyed with copper and iron - - 6 pieces.
6. Gold alloyed with $\frac{3}{4}$ of copper and $\frac{1}{4}$ of tin - 6 pieces.
7. Gold alloyed with an equal part of copper - 6 pieces.

The specific gravity of each of the foregoing series of six pieces was then taken, with every possible precaution; and afterwards the pieces were fixed in a machine, so that three of each series were opposed to the other three which were of a similar quality. The machine was then put in motion; and these pieces were made to rub against each other for a considerable time, or till 200300 revolutions had been performed; after which, the pieces were removed, and the specific gravity of each series was again ascertained.

The following comparative statement will show, that a very evident diminution of the original specific gravity had taken place, in consequence of this long continued friction.

Quality.	Specific gravity before friction.	Specific gravity after friction.
1. Gold of 23 car. $3\frac{3}{4}$ grs. fine -	19,277	19,171
2. Gold alloyed with silver -	18,092	18,055
3. Gold alloyed with silver and copper - - -	18,184	18,182
4. Gold alloyed with copper -	18,053	18,014
5. Gold alloyed with copper and iron - - - -	17,151	17,095
6. Gold alloyed with copper and tin - - - -	17,607	17,581
7. Gold and copper, in equal parts	12,142	12,139

The proportion of gold in the foregoing pieces exceeded, in general, the standard quantity; but that circumstance did not interfere with the principal object of these experiments, which will also be corroborated by the result of the subsequent experiment.

Experiment II.

Twelve pieces of fine copper, similar in size to those of gold which were employed in the former experiment, were weighed hydrostatically, altogether, and were afterwards placed in the machine, so that six were made to rub against six. After 22200 revolutions, they were taken out, and their specific gravity was again accurately examined.

A very apparent diminution was found to be the result, for the specific gravity was,

Before friction.	After friction.
8,785	8,283.

Considering, therefore, that this diminution of specific gravity was found in each of the foregoing series, as well as in the

present experiment, after the pieces had been subjected to long continued friction, there cannot be any doubt but that this is a general effect, which probably arises from the pieces having suffered expansion, in consequence of heat generated during the friction; and, (similar to what has been observed in pyrometrical experiments,) that these pieces of metal did not, upon the cessation of friction, return precisely to their original size and specific gravity.

Among the other less powerful causes which produce some alteration in the specific gravity of gold, the processes of rolling, and of annealing, may also be enumerated; for, in the course of these experiments, I have always found, that the specific gravity of the bars, &c. was in a small degree increased by rolling, and that the contrary effect was produced by annealing.

The specific gravity of gold, 23 car. $3\frac{3}{4}$ grs. fine, when rolled and stamped without being annealed, I found to be 19,277; but, when the same was annealed, the specific gravity was 19,231.

I am, however, inclined to believe, that annealing had reduced the specific gravity to much less than is here stated; and that the subsequent operation of stamping had, in some measure, compensated the effects of annealing. For, it may be recollected, that in the experiments lately mentioned, it was proved, that the specific gravity of the pieces which had not been annealed, was reduced, by long continued friction, from 19,277 to 19,171; an effect surpassing that which resulted from annealing by ,060 (19,231 − 19,171 = ,060); and, if heat was the cause, the reverse might have been expected, inasmuch as the annealing heat exceeded that which was produced by friction; but,

as this was not the case, I am induced to be of opinion, that the specific gravity was again increased, by the subsequent stamping of the annealed pieces.

In addition, therefore, to those causes of variation in specific gravity which are the immediate consequences of hydrostatical operations, such as, the different height of the column of water, and the changes of temperature to which it is exposed during the experiments, the following, as far as they concern metallic substances, may be enumerated.

1. Imperfections in the interior of the mass, which are produced during the processes of melting and casting.

2. The difference of density in parts of the same mass, resulting from the quality and quantity of the metal, from the nature of the mould, from the more or less vertical position of it, and from the height of the column or bar of metal which is cast.

3. The unequal distribution of the metal, or metals, employed as an alloy, throughout the mass intended to be alloyed.

4. The peculiar effects which certain metals produce, when used singly or conjointly as alloys, and which are very different from the results of calculation.*

5. Heat, whether produced by friction or excited in any other manner.

* "There can be no doubt but that the effects of compound alloys are, in general, very different from those of each metal separately considered; and that such metallic combinations or compound alloys, like neutral salts, and many other compounds, have peculiar properties, which act variously upon the metals to which these compound alloys are added. A great number of accurate experiments are, however, requisite to elucidate a question so intricate.

It may here be also observed, that the peculiar properties of compound alloys, prove them to be real chemical combinations.

As, therefore, the specific gravity of metals is liable to be influenced by such a variety of causes, it is almost in vain to expect absolute precision, in the results of experiments made by different persons; but, at the same time, it may be observed, that by proper care and attention to the above circumstances, a degree of accuracy may be attained, sufficient to answer almost every useful purpose, although, from what has been said, it must appear improper to form opinions upon small fractional variations. By the experiments which I made, with every possible precaution, upon separate and intire ingots of gold, reduced to standard by silver, by silver and copper, and by copper alone, when cast in an iron mould like a cupel, it appeared, that the specific gravity of each of these kinds of standard gold is as follows.

Gold made standard by silver	- -	17,927
Gold made standard by silver and copper	-	17,344
Gold made standard by copper	- -	17,157.

Now, as our gold coin commonly contains silver as part of the alloy, and as at different times this proportion of silver must have been various, and even considerable; particularly when the gold of Portugal, which is alloyed with silver, was brought to the Mint, it naturally follows that, exclusive of the many other causes of variation which have lately been enumerated, the specific gravity of our standard gold must occasionally be different, according to the relative proportions of silver and copper which compose the alloy;* and, as the specific gravity of gold made standard by silver is, in the ingot cast under the above circum-

* The first guineas which were coined, or those of CHARLES II. and JAMES II. were generally alloyed with standard silver; but the coins of the subsequent reigns have been alloyed with copper, added to compensate the deficiency of alloy, or of silver in the gold.

stances, 17,927, while that of gold made standard by copper is only 17,157, so, according to the relative proportions of these two metals, when united in the alloy, the specific gravity of the standard gold may vary between the two extremes of 17,927 and 17,157, although the real quality or value of the standard gold remains unchanged; and indeed, when some allowance is also made for small variations arising from other causes, the range of the different specific gravities of gold made standard by silver and copper, may be considered as nearly extending from 18 to 17.

The following Table is intended to show the various Statements of different Authors, respecting the specific Gravity of fine and of standard Gold.

TABLE III.

Fine gold.	Specific grav.	Names of Authors.
Fine gold - -	19,640	WARD, COTES, MUSSCHENBROEK.
A medal, esteemed to be nearly fine gold -	19,636	CASWELL.
Fine gold - -	19,300 to 19,400	LEWIS.*

* Dr. LEWIS asserts, that when he had refined gold to the greatest degree of purity which he believed it capable of being brought to, and when the same had been well hammered, he, from many trials, found the specific gravity between 19,300 and 19,400. Phil. Comm. of Arts, p. 41.

From every circumstance, therefore, it may be concluded, that 19,640, which has been stated as the specific gravity of fine gold, by WARD, COTES, MUSSCHENBROEK, and others, is merely hypothetical.

TABLE III. *(continued.)*

Fine gold.	Specific grav.	Names of authors.
Gold of 24 car. hammered	19,361	BRISSON.
Gold of 23 car. $3\frac{3}{4}$ grs. stamped and rolled	19,277	HATCHETT.
Gold of 24 car. from an ingot - -	19,258	BRISSON.
Gold of 24 car. from another ingot - -	19,257	BRISSON.
Fine gold, hammered -	19,207	ELLICOT.
Gold in the ingot, said to be fine, and again refined with antimony -	19,184	ELLICOT.
Gold of 23 car. $3\frac{1}{2}$ grs. in the bar - - -	19,172	HATCHETT.
The ingot already mentioned by Mr. ELLICOT, before it was refined with antimony - -	19,161	ELLICOT.
A medal of the Royal Society, reported fine gold	19,158	GRAHAM.
A medal of Q. ELIZABETH	19,125	CASWELL.

TABLE III. *(continued.)*

Fine gold.	Specific grav.	Names of authors.
A medal of Queen MARY	19,100	CASWELL.*
Gold - -	19,081	FAHRENHEIT.
Aurum purum - -	19,000	BACON (ex Hyp.)
A coin of ALEXANDER	18,893	CASWELL.
Gold - - -	18,806	REYNOLDS.
Gold - - -	18,750	VILLALPANDUS.

Standard gold.	Specific grav.	Names of authors.
Gold of 22 car. or standard	18,888	CASWELL, WARD, COTES, and MUSSCHENBROEK.
An old JACOBUS, supposed to be the sceptered broad piece	18,375	HARRIS.
A five-guinea piece of King JAMES II. 1687 -	17,933	GRAHAM.
Guineas, 10, weighed together - - -	17,800	DAVIES.

* Dr. DAVIES observes, that these medals of Queen ELIZABETH and Queen MARY were undoubtedly the large Sovereigns of those queens, which were of the old standard of England, or of gold appointed to be of 23 car. $3\frac{1}{2}$ grs. fine. See Tables of specific Gravities, extracted from various Authors, with some Observations upon the same; by RICHARD DAVIES, M. D. Phil. Trans. Vol. XLV. page 416.

TABLE III. *(continued.)*

Standard gold.	Specific grav.	Names of authors.
Guineas, on a mean of seven trials upon those of different reigns - -	17,726	ELLICOT.
A guinea - -	17,629	BRISSON.*
A piece of gold coin of the Commonwealth -	17,625	HARRIS.
Guineas, two new ones -	17,414	HAUKSBEE.
Gold made standard by silver, in the ingot -	17,927	HATCHETT.
Gold made standard by equal parts of silver and copper, in the ingot -	17,344	HATCHETT.
Gold made standard by fine Swedish copper, in the ingot	17,157	HATCHETT.

* From the whole of the experiments related in this Paper, it must be evident, that small fractional variations in the specific gravity of gold coin do not merit attention; it is not safe, therefore, to draw any general inference from a single experiment, made upon one piece, or even upon a small number of pieces.

Mr. BRISSON examined the specific gravity of a single guinea, which he found to be 17,629; and, as he had previously ascertained the specific gravity of the gold coin of France to be 17,647, he says, "this proves, (contrary to the received erroneous idea,) "that the specific gravity of the French gold coin is greater than that of England." *Pésanteur specifique des Corps*, p. 9.

But this conclusion of Mr. BRISSON cannot be admitted; for, even the different proportions of silver and copper in the alloy, (exclusive of other causes,) may produce variations in the specific gravity of standard gold, between 17,927 and 17,157.

However respectable the names of some of the foregoing authors may be, there is much reason to believe, that the specific gravity of fine gold, in the two first instances, has been too highly estimated; and, as to standard gold, there cannot be any doubt but that some error must have been the cause which induced CASWELL, WARD, COTES, and MUSSCHENBROEK, to rate it at 18,888; and HARRIS to state the specific gravity of the JACOBUS at 18,375.

What this error was, cannot now be determined; but, if the operations were accurately performed, and, considering the eminence of the persons concerned, this can scarcely be doubted, we must conclude that too small a proportion of alloy was present in both cases; for this appears to be very probable, from the general result of the whole of the preceding experiments.* Some such cause of error must have therefore prevailed, in the two first cases of standard gold contained in the foregoing Table; and it is absolutely necessary that this should be strongly pointed out, lest any one should fall into a mistaken notion, which has but too commonly been received in this country, and which has injuriously and unjustly been believed on the Continent, to the detriment of the British Exchange. The erroneous idea to which I allude is, the belief that the standard gold of the present reign is inferior to those which have preceded it; the real fact is, however, precisely the reverse, as the following extract, from the Report of Messrs. GARBETTS to the Lords of the Treasury, in 1782, will sufficiently prove.

* It is very probable, that the alloy was as much too abundant, in many similar pieces of the same coinage, as it was deficient in those here mentioned; for, it is certain, that the gold coins of JAMES II. and of CHARLES II. were, in the aggregate, much inferior to the present standard, as the annexed extract from Messrs. GARBETTS' Report sufficiently evinces.

Extract from Messrs. GARBETTS' *Report.*

" We had reason to believe that our gold coin was not esti-" mated, at foreign Mints, of the same fineness which our " standard declares it at, viz. 22 parts fine gold, and 2 parts " alloy; and, upon intimating this circumstance to the King's " and Master's Assayers, we were informed that a plan had been " settled, prior to the recoinage, for ascertaining the actual " fineness of the coin; and that guineas of every separate reign " had been melted into ingots of 15 pounds each, without in-" termixing the different reigns; that, from the contrary ends " of each ingot, they had made assays, which so nearly accorded, " as not to leave a doubt but the coins were worse than stand-" ard. The King's Assayers record of them was as follows. " *Viz.*

				s.	*d.*
" CHARLES II.	26	Tr. grs. in a lb. worse than standard	=	9	$10\frac{1}{4}$ p'c
" JAMES II.	30	——— Ditto - -	=	11	$4\frac{1}{2}$
" WILLIAM III.	13	——— Ditto - -	=	4	11
" ANNE - -	7	——— Ditto - -	=	2	$7\frac{1}{2}$
" GEORGE I.	6	——— Ditto - -	=	2	$3\frac{1}{4}$
" GEORGE II.	3	——— Ditto - -	=	1	$1\frac{1}{2}$
" GEORGE III.	standard - -	standard - -		standard.	

" The accuracy of these assays was farther confirmed, by " nearly the same average of worseness being found upon more " than 170000 guineas, taken promiscuously from those reigns.

" In this place it should be observed, that if a pound of gold " coin does not vary more than 40 Troy grains in fineness, and " in weight, or in both together, it is allowed by the Mint in-" denture to pass as standard.

" During Lord CADOGAN's mastership, the average of weight " hath been only 2 grains 156 decimals lack per lb. which was

"paid by the moneyers at the scale; and, in upwards of 40000 "assays from the specimens of coin taken at the pix, (of "twenty-eight millions sent into circulation,) only one hath "deviated in fineness 3 grains in the pound; and, from the "public trial of them by the Goldsmith's Company, there hath "not been recorded more than 4 grains error in weight, and no "deviation in fineness.

"The Master of the Mint, therefore, might have varied in "fineness 36 Troy grains in a pound, or 13*s.* 7¾*d. per cent.* "without being liable to censure, if it did not appear he had "done it by design. This is sufficient to show the impropriety "of allowing a latitude of 40 Troy grains in a pound, for error "in fineness, or in weight together, or in either; and which "has so operated as to make our guineas of less value than we "declare them, and to be estimated, as we are informed, at the "Dutch Mints, 10 grains worse, and at Paris 15 grains Troy "worse; nor do they make any difference, either in Holland or "in France, between our present King's guineas and those of "former reigns.

"How far this deficiency affects the par of exchange in "money, and the Course of Exchange in bills, we submit to "consideration, as a matter of great importance."

Since the preceding pages were written, in which I have stated the numerous causes which tend to produce variations in the specific gravity of gold made standard by silver and copper, I have been induced to examine several of the English gold coins, and particularly those of the present reign.

The results of this examination, contained in the annexed table, fully confirm my former sentiments, especially in respect [to the] impropriety of estimating the value of the coin of a

country, by insulated experiments on the specific gravity of a few pieces; for, a certain variation in the specific gravity of coin, independent of any alteration in its real value, is almost, if not absolutely, unavoidable.

Specific Gravity of some of the English gold Coins, at Temperature 60° of FAHRENHEIT.

TABLE IV.

Reign.			Date.	Specific gravity.
CHARLES	II.	a five-guinea piece - -	1681	17,825.
JAMES	II.	a two-guinea piece - -	1687	17,634.
WILLIAM	III.	a five-guinea piece -	1701	17,710.
GEORGE	I.	a quarter-guinea - -	1718	16,894.
GEORGE	II.	a guinea - - -	1735	17,637.
———		a two-guinea piece -	1740	17,848.
GEORGE	III.	one guinea - - -	1761	17,737.
———		one guinea - - -	1766	17,655.
———		one guinea - -	1774	17,726.
———		one guinea - - -	1775	17,698.
———		one guinea - - -	1776	17,486.
———		one guinea - -	1777	17,750.
———		one guinea - - -	1782	17,202.
———		one guinea - - -	1786	17,465.
———		one guinea - - -	1788	17,418.
———		five guineas - - -	1793	17,712.
———		ten half-guineas - -	1801	17,750.
———		15 seven-shilling pieces* -	1802	17,793.

* Supposing guineas, half-guineas, and seven-shilling pieces, to be made from the same metal, there is reason to expect (in a given comparative sum of each) an increase of specific gravity in the smaller coins, as a natural consequence of rolling, punching, annealing, blanching, milling, and stamping, the effects of which must become more evident, in proportion to the number of the small pieces required to form a given sum of the larger coins.

The average specific gravity of our gold coin, at the present time, may probably be estimated at 17,724.

SECTION III.

ON THE COMPARATIVE WEAR OF GOLD, WHEN ALLOYED BY VARIOUS METALS.

The comparative wear of gold, especially when in the form of coin, has never yet been ascertained; the opinions concerning it are therefore various. The most prevalent idea appears to be, that pure or ductile gold suffers more in a given time, under equal circumstances, than that which is of a harder quality.

Supposing this fact to be well established, it would not be difficult to render gold as hard as could be desired; for, as certain metals, when employed in equal proportions, cause gold to become of very different degrees of hardness, it would be easy even to make gold perfectly hard and brittle, without changing the standard proportion of alloy, provided that such extreme hardness was compatible with the process of coining.

But the question, whether ductile or hard and brittle gold sustains the greatest loss by wear, under equal circumstances, has by no means been fully determined; and Mr. HARRIS appears to have considered hard metal as the most liable to suffer, it being, when compared to that which is pure and soft, more brittle and less tenacious.*

Gold, when in the form of coin, appears to be generally exposed to three varieties of friction, *viz.*

1st. Friction between pieces of gold coin of a similar or of a different quality.

* An Essay upon money and Coins, 1758, Part II. page 117.

2dly. Friction of gold coin against coin of other metals, such as silver and copper.

3dly. The friction which gold coins of various qualities suffer, when exposed to the action of certain substances, such as the particles or filings of metals, gritty powders, &c.

The consideration of these different modes of wear, points out the best method to be pursued in an experimental investigation.

The whole of the experiments which compose this section may therefore be divided into three subordinate series; the two first of which have been directed to the consideration of that part of the diminution of the coin which arises from the rubbing of one piece of metal against another; while,

The third of these subordinate series was intended to show the comparative power of gold, differently alloyed, to resist abrasion from sand or other gritty powders.

In the first set of experiments, 28 pieces of coin were fixed to a frame, and over each of them was placed another piece of coin, which was pressed against it by a weight. These upper pieces were all attached to a second frame, so that, by means of the motion communicated thereto by cranks, each upper piece was made to move about $\frac{1}{8}$ of an inch backwards and forwards on the lower one. This mode of experiment afforded an opportunity of trying the comparative diminution of gold differently alloyed, both when rubbed against pieces of the same and of a different alloy; and also of examining the difference of wear between pieces with plain and with stamped faces.

In the second series, 200 pieces of gold, differently alloyed, were inclosed in a wooden box, of a cubic figure, which was kept constantly turning round, till, by the repeated rubbing and striking of the pieces against each other, and against the sides of

the box, they were found to be sensibly diminished. This, like the experiments of the first set, was intended to show the comparative diminution of gold differently alloyed; but, whereas that shewed the effect of rubbing only, this shewed the joint effect of rubbing and striking, and was intended to imitate (although in a more violent degree) the effect produced upon coin by pouring it out of one bag or drawer into another.

The experiments of the third set were made by pressing the pieces to be examined against the rim of a flat horizontal wheel, by means of equal weights, so that, by turning the wheel round, they all suffered an equal degree of friction. That part of the wheel against which the pieces rubbed, was sprinkled or coated with some kind of powder, which was occasionally varied.

The above statement will convey a general idea of the manner of making the experiments; but, that the whole may be more fully comprehended, the following description of the instruments has been added by Mr. CAVENDISH.*

DESCRIPTION OF THE INSTRUMENTS.

It has been already observed that, in the first series of experiments, 28 pieces of coin were fixed to a frame, and that over each of them was placed another piece, which was pressed against it by a weight; and that these upper pieces were all connected to a second frame, so that, in consequence of the motion communicated thereto by cranks, each upper piece was rubbed backwards and forwards upon that which was under it.

Fig. 1, (Plate II.) represents a plan of this instrument; and Fig. 2 is a vertical section of it, drawn parallel to the line AB.

* The instruments were made by Mr. CUTHBERTSON, of Poland-street, who also took the care of them during the experiments which were made at his house.

The upper frame, or that to which the upper pieces of coin are connected, is of brass, and consists of four bars, Fig. 1, AB, B*b*, *ba*, and *a*A, with three cross bars C*c*, C*c*, C*c*.

The lower frame consists of a board, placed immediately under the upper frame, and is expressed in Fig. 2, by the letters LL.

The upper frame is supported by two vertical boards, extending the whole length of the sides B*b* and A*a*, so that the ends of them are seen in Fig. 2, and are denoted by the letters DD, DD. These boards are fastened to the upper frame, and to the table upon which the apparatus stands, by hinges, so that the upper frame can move freely in the direction BA, but can have no motion in the direction perpendicular thereto. These vertical boards are omitted in Fig. 1; for, as the intention of this description is not to give a detail of all the parts of the instruments, but only to explain their manner of acting, I have taken the liberty to omit such parts as tended to produce an intricacy in the figures, without being necessary to this object.

The disposition of the pieces of coin on the frames, is represented in Fig. 1. N*nn* denote one of the connecting pieces, by which the upper pieces of coin are connected to the upper frame, and in which the small circle represents the position of the coin; the large circle is the part which supports the weight, and *nn* the part by which it is connected to the upper frame.

To avoid confusion, neither these connecting pieces nor the pieces of coin are represented in Fig. 2; but, instead thereof, a section of one of these pieces is given in Fig. 3, upon a larger scale.

In this figure, LL, is the lower frame, and C one of the bars of the upper frame; *y* is one of the lower pieces of coin, which

is bedded and fixed firmly in a brass socket x, fastened to the lower frame; u is the piece of coin to be rubbed against it, which, in like manner, is fixed in another brass socket w; Nn is the connecting piece, by which this socket is connected to the bar C of the upper frame. This piece turns on pivots, in two studs n, fixed to the bar C, so that it can turn freely on those pivots in a vertical direction, but cannot be perceptibly shaken horizontally.

Z is the weight by which this connecting piece is pressed down; it is round, and is placed with its centre exactly over that of the socket w.

It must be observed that, in the construction of this machine, three things principally demanded attention.

1st. That the pieces of coin should all move equally.

2dly. That they should all be pressed against the lower pieces by the same weight. And,

3dly. That they should bear flat against them.

As to the first requisite, it is evident that the pieces must all move alike, excepting so far as proceeded from the springing of the parts of the machine, or from the shake in its joints, both of which were very small.

Secondly, as the connecting pieces move freely in a vertical direction, it is clear that the force with which the upper piece of coin is pressed against the lower one, depends only on its own weight, on that of the socket w, on that of the connecting piece Nn, and on the weight Z by which it is loaded; so that the second requisite is thus easily obtained.

Thirdly, the connecting piece Nn bears against the socket w only by the pin p, which enters into a hole in the centre of the socket, so that the two pieces must necessarily bear flat against

each other; but, as this pin alone would not have prevented the socket from turning round on its centre, two other pins $\Pi\,\Pi$ were fixed into the connecting piece, and entered into slits made in the socket near its circumference, allowing no more shake than was necessary to prevent it from sticking; and thus the motion round the centre was effectually prevented.

It may be observed, that the pieces might have been made to bear flat against each other by fixing the sockets *w* in gimbals; but, as the method above described was effectual, and much easier made, it was preferred.

It may be also remarked, that the breadth of the bars C*c*, as represented in Fig. 1, is not sufficient to prevent them from springing considerably; for this reason, a method of strengthening them was employed, which answered the purpose perfectly well, but is omitted in the drawing, as it could not be easily represented.

It was at first intended, that the lower frame should have remained fixed, and that only the upper one should have moved; but, in a previous trial, in which two pieces of metal were rubbed backwards and forwards upon each other in the same line, with a view to discover what weight would be necessary to make the pieces wear tolerably fast, I found that for a time they diminished slowly, but that little furrows or gullies were soon worn in them, and that then the diminution was rapid. I also observed, that the gullies in the upper pieces corresponded to those in the lower ones; so that it was impossible that the pieces of metal should touch each other in those places where the diminution was most rapid, and consequently the gullies must have been formed by the particles of metal which had been abraded, and which subsequently had become accumulated.

It seemed to me, that the most probable way to prevent the little furrows or gullies from being thus formed, would be, to construct the instrument in such a manner, that the direction in which the pieces rubbed upon each other should continually vary. The following contrivance was therefore adopted, by which the pieces were prevented from rubbing together twice in the same direction.

In this method, the lower frame, as well as the upper, is supported on two moveable vertical boards; but, whereas the boards supporting the upper frame are placed parallel to B*b*, in consequence of which the frame can move only in the direction BA, these are placed parallel to BA, so that the frame can move only in the direction B*b*.

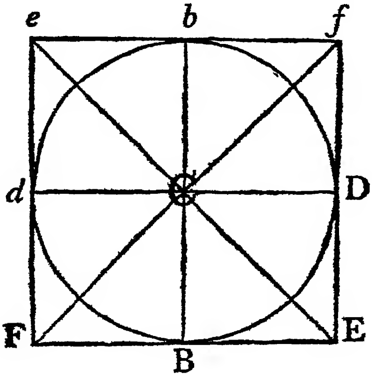
EE is the axis by which the upper frame is moved: this turns in fixed sockets at SS, and is turned at each end into the form of an eccentric circle, which acts as a crank; so that, by means of the levers EK, which at one end turn on these eccentric circles, and at the other end turn on joints fixed to the upper frame, this frame is made to move $\frac{1}{4}$ of an inch, in the direction BA, during one half of the revolution of the axis, and as much in the contrary direction, during the other half revolution.

ee is an axis of the same kind, serving to move the lower frame. HH is a windlass, which turns these two axes by means of the toothed wheels F, *f*, which work in the toothed wheels G, *g*, fastened to the axes EE and *ee*. TTTTT is the table upon which the apparatus stands.

The wheel F has 90 teeth, *f* has 75, and G, *g*, have each 20; so that the axis EE makes six revolutions while *ee* makes five; and, at a medium, these axes make about four revolutions to one

of the windlass. A counter is placed so as to show the number of revolutions of the windlass.

If the two frames had performed their vibrations in the same time, no advantage would have been gained, for the pieces of coin would still have moved upon each other always in the same



line; but, as their vibrations are performed in different times, the effect is quite different; for, let C, in the annexed figure, be the centre of one of the pieces in the lower frame. Draw the lines B*b* and D*d* in the directions of the motion of the lower and upper frame, and equal to the space which those frames describe in one semi-revolution of the cranks, and complete the square of *ef* EF. Then, if the upper frame is moving with its greatest velocity in the direction D*d*, at the same time that the lower one is moving with its greatest velocity in the direction B*b*, the motion of the upper piece on the lower one will be in the diagonal *f*F; but if, at that time, the lower frame is moving with its greatest velocity in the contrary direction *b*B, the motion will be in the other diagonal E*e*.

If one frame is moving with its greatest velocity, while the other is at the extremity of its vibration, the motion will be in the circumference of the circle *b* D B *d*, inscribed within the square; and, in the intermediate cases, it will be in the circumference of an ellipsis, which is inscribed in the same square, and whose axes are in the diagonals *e*E and *f*F, but in which the proportion of the axes is continually changing; that axis which is placed in F*f* being sometimes the greatest, and at other times the least.

This contrivance, therefore, effectually prevented the pieces from moving upon each other always in the same line; and it seems also to have much diminished the disposition which they had to wear in gullies, but not intirely; for, from the following experiments it appears, that still some few particles would become occasionally collected, and then acted as a grinding powder, which accelerated the wear of the pieces. This was observed particularly to happen to the pieces of gold alloyed with an equal proportion of copper, and to the pieces of copper, which were also more frequently worn in furrows or gullies, than the other pieces of more ductile metal.

The motion of the pieces of coin upon each other, is greater than it would have been if only one frame had been made to move, nearly in the proportion of 3 to 2; so that the whole motion of the pieces, in each semi-revolution of the axes EE or *ee*, is about $\frac{3}{8}$ of an inch, and therefore it is about three inches in each revolution of the windlass.

The instrument employed in the second series of experiments, is so simple as not to require any drawing. It consisted only of a cubical box of oak, which measured 8 inches each way, within side. This box was moved by the axis EE of the former instrument, which was passed through the middle of two opposite sides, and was fixed in that position.

Fig. 4. represents a plan of the instrument used in the third series of experiments. *aaa* is a horizontal table, turning upon a vertical axis; and BBB*bbb* is a fixed frame surrounding it.

The pieces of coin are fastened to this fixed frame, by the same connecting pieces which were formerly employed, and are pressed down also by similar weights. The diameter of that part of the wheel against which the centres of the pieces of coin

are pressed, is 29 inches; so that, while this wheel makes one revolution, the pieces are rubbed against it through the whole circumference of this circle, that is, through $91\frac{1}{10}$ inches.

A shallow groove *ggg* is cut in this wheel, in that part against which the pieces are pressed, in order to confine the powders employed in the experiments; and the number of revolutions of the wheel are marked by a counter.

By the help of the instruments above described, it was proposed to determine, as accurately as possible,

1st. The comparative wear of soft and of hard gold.

2dly. Whether coins with flat or with raised surfaces suffer the greatest loss by friction, when subjected to it under similar circumstances.*

It is scarcely necessary to observe, that rigorously exact results could not be expected in all the minutiæ of experiments like the present; for, many circumstances, apparently but trivial, produced almost unsurmountable obstacles; but, nevertheless, these did not impede the essential objects from being investigated, and determined, in a manner sufficiently satisfactory.

Before the experiments are described, it will be proper to add, that, to obviate the irregular effects which would be produced by the inequality of the impressions usually employed for coins, Mr. Cavendish suggested a die, which was executed by Mr. Pingo, and which consisted of round prominencies regularly disposed over the surface, so that the effects which this

* Although coins with protuberances on their surfaces, have been generally supposed to suffer more by friction than those which are flat, yet, as this opinion has been questioned, and as several objections have been made to it by intelligent persons, it was thought expedient that the decision of the question should form part of the present investigation

impression produced, during friction, were uniformly the same in every direction.

The first experiments were intended to ascertain the different wear of gold made standard by various metals; and the pieces were rubbed against each other by means of the first-described apparatus, which I shall call No. 1.

Some preparatory experiments were also made, to try the effects of this machine, as well as to determine, in some measure, the comparative wear of gold made standard by copper, of a mixture of gold and copper in equal proportions, and, lastly, of copper.

Experiment 1.

Twelve pieces of the standard gold were first examined, and were placed so that six were opposed to six.

The brass frame, in which each upper piece was fixed, weighed 1604 grains; and it was found necessary to add to each a weight of lead, equal to 19825 grains; so that the pieces were rubbed against each other under the pressure of 19825 + 1604 = 21429 grains = 3 lb. 8 oz. 12 dts. 21 grs.*

The machine was then put in motion, until the index showed that 286690 revolutions had been performed; and, as a double crank acted during each revolution, the pieces were rubbed against each other alternately, in opposite directions, 573380 times, being twice the number of the revolutions.

The twelve pieces of standard gold, being taken out, were weighed, and were found to have lost 8,60 grs.

* This weight may appear to be very considerable; but it was not employed until repeated trials had proved the extreme difficulty, and almost impossibility, of producing any perceptible effect within a moderate period of time; and, even with this weight, the experiments were found to be exceedingly tedious. The only evil which resulted [illegible] such a pressure was, that the comparative wear of the fine gold appeared much [illegible] considerable than would have been the case, if a small weight could have been [illegible]; some observations will therefore be found in the subsequent pages, which [illegible] the necessity of making an allowance for this circumstance.

Experiment II.

This experiment was made upon twelve pieces of gold combined with an equal proportion of copper. The faces which were opposed were flat, and without any impression. After 70640 revolutions, these pieces had lost 103,11 grs.

Experiment III.

Twelve pieces of fine copper, perfectly flat, and not stamped, were next placed in the machine, and were taken out after 22200 revolutions; they had then lost 174,80 grs.

From these preliminary experiments it appears,

1st. That pieces of gold made standard by $\frac{1}{12}$ of copper, when rubbed against each other, suffer less than gold much debased by copper, or in which the latter metal is in equal proportion to the gold. And,

2dly. That pieces of gold alloyed with an equal quantity of copper, when rubbed against each other, suffer less than pieces of copper which are subjected to a similar process.

These essential objects being thus ascertained, the following experiment was made.

Experiment I.

Forty-eight pieces of gold, variously alloyed, which were perfectly flat and smooth, were fixed in the machine No. 1.

In this experiment, six pieces of each kind of gold were employed, and were so arranged, that three of each were made to rub against three of a similar quality; and the loss produced by friction, was afterwards estimated upon the whole of the six pieces.

The annexed Table will show the comparative loss sustained by the different kinds of gold.

TABLE I.

Total number of revolutions, 200300.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
1. Gold made standard by copper - -	844,90	844,90	—
2. Gold reduced to 18 carats by copper - -	747,60	747,60	—
3. Gold made standard by copper and silver -	829,20	829,10	,10
4. Gold made standard by silver - - -	937,20	937,10	,10
5. Gold 23 car. $3\frac{3}{4}$ grs. fine	854,0	849,80	4,20
6. Gold made standard by tin and copper - -	846,90	831,60	15,30
7. Gold made standard by iron and copper - -	825,10	803,50	21,60
8. Gold alloyed with an equal proportion of copper -	615,68	549,90	65,78

According to this statement, it appears, that fine gold of 23 car. $3\frac{3}{4}$ grs. suffered more by friction, under the above described circumstances, than gold made standard either by copper, by silver and copper, or by silver; but that this fine gold of 23 car. $3\frac{3}{4}$ grs. suffered less by wear than gold made standard by tin and copper, or by iron and copper; and, lastly, that copper, although it appears to be beneficial when in the proportion of $\frac{1}{12}$, and sometimes when it even amounts to $\frac{3}{12}$, yet, if employed in a larger proportion, for example, when equal in the quantity of gold, it then becomes highly detrimental.

for it not only much injures the colour of the precious metal, but also renders it extremely susceptible of the effects of friction. The presence of tin, or iron, appears also to render standard gold more liable to wear, than when the alloy consists only of copper, or of silver. So rapid was the loss of the pieces composed of equal parts of gold and copper, and of the others in which iron was present, that it was found necessary to remove the former, as well as those pieces which contained iron, after 105480 revolutions had been performed. The pieces containing tin were worn so thin, after 189000 revolutions, that they also were obliged to be taken out. As, therefore, the whole of the others sustained 200300 revolutions, it may be concluded, that the comparative loss of the pieces which were taken out, although very considerable, would have been much greater, had it been possible to have kept them in the apparatus during the whole period of the experiment.

The preceding experiment was made upon smooth, flat, unstamped pieces; it was therefore thought necessary to repeat it, in some measure, upon those which had been stamped by the die already described. In the following experiment, there was also a small variation, in respect to the quality of the series which were examined; for, the pieces composed of gold and copper in equal proportions were omitted, and some pieces of standard silver, and some of fine copper, were added.

Experiment II.

In this experiment, as in the former, pieces of similar quality were opposed to each other; and, in general, every circumstance was the same, excepting that the pieces were stamped, that the number of revolutions amounted only to 20680, and that all

the pieces remained in the apparatus till the experiment was finished.

TABLE II.

Number of revolutions, 20680.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
1. Gold made standard by copper - - -	846,90	846,30	0,60
2. Gold made standard by copper and silver -	834,80	833.60	1,20
3. Gold made standard by silver - -	940,30	936,80	3,50
4. Standard silver - -	518,70	515	3,70
5. Gold 23 car. $3\frac{3}{4}$ grs. fine -	846.40	841.80	4,60
6. Gold reduced to 18 carats by copper - -	745,80	741	4,80
7. Gold made standard by iron and copper -	825,60	818	7,60
8. Gold made standard by tin and copper - -	849,40	835,60	13,80
9. Copper - - -	496,90	450,60	46,30

Upon comparing the result of this experiment with that of the former, it may in like manner be observed, that gold made standard by copper, or by silver and copper, or by silver, suffered the least by wear; after which, standard silver, and then gold of 23 car. $3\frac{3}{4}$ grs. together with the others enumerated in the Table, were progressively more affected; and as, in the first experiment, the greatest loss was sustained by gold alloyed with

an equal proportion of copper, so, in this last experiment, the copper pieces suffered the most considerable diminution.

It must however be also remarked, that, contrary to the former experiment, gold reduced by copper to 18 carats, lost more than gold of 23 car. $3\frac{3}{4}$ grs.; and gold alloyed with tin and copper, lost more than that which was alloyed with copper and iron; but, in general, the coincidence of the results of the two experiments, appears to be as satisfactory as could with reason be expected.

Lastly, it appears, upon comparing the effects produced, with the number of revolutions employed in the two experiments, that we have a proof of the increase of wear which attends the friction of raised or embossed surfaces.

Hitherto, the effects produced by the friction of pieces of a similar quality only had been examined; but, in order to ascertain the comparative wear which would be occasioned by rubbing pieces of a similar and of a different quality against each other, by one operation, the following experiment was made.

Experiment III.

This experiment was made upon 54 unstamped pieces of gold, the different qualities of which are expressed in the annexed Table; and it is necessary here to observe, that standard gold is always to be understood from the terms gold alloyed with silver, gold with silver and copper, &c. excepting when other proportions are expressly stated.

TABLE III.

Number of revolutions, 229040.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
1. Gold 23 car. $3\frac{3}{4}$ grs. -	142,50	139,50	3,0
2. Gold 23 car. $3\frac{3}{4}$ grs. -	140,50	137,20	3,30
3. Gold alloyed with silver -	153,60	153,60	—
4. Gold alloyed with silver -	158,90	158,80	0,10
5 Gold with silver and copper	137,40	131	6,40
6. Gold with silver and copper	136,80	123,50	3,30*
7. Gold with copper - -	135,30	135,30	—
8. Gold with copper -	135,40	135,30	0,10
9. Gold with iron and copper	134	116,80	17,20
10. Gold with iron and copper	133,80	119,80	14,0
11. Standard silver -	84,50	84,40	0,10
12. Standard silver - -	84,50	84,40	0,10
13. Copper - - -	84,70	34,60	50,10
14. Copper - -	82,30	43,60	38,70
15. Standard silver -	82,90	68	14,90
16. Copper - - -	82,40	46,60	35,80
17. Gold made standard by copper - - -	117,10	83,40	33,70
18. Copper - -	66,80	23,50	43,30
19. Gold with copper - -	134,40	129,20	5,20
20. Standard silver -	83,30	75,10	8,20
21. Gold with copper -	138,70	138,60	0,10
22. Gold with silver - -	152,90	152,80	0,10
23. Gold with copper -	136,20	132,90	3,30
24. Gold with iron and copper	137,10	130	7,10

* These two pieces slipped out of the sockets of the machine, and were so much [illegible], that it would be improper to consider the loss mentioned in the Table as that [illegible] they would have suffered by regular friction.

TABLE III. (*continued.*)

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
25. Gold with copper -	142	141.90	0,10
26. Gold with silver and copper	137,80	137,80	—
27. Gold 23 car. 3¾ grs. -	137,80	137,70	0,10
28. Standard silver -	84,10	84	0,10
29. Gold 23 car. 3¾ grs. -	138,10	138,10	—
30 Gold with silver -	155,40	155.40	—
31. Gold 23 car. 3¾ grs. -	137,60	136,10	0,50
32. Gold with copper - -	144,20	144,60*	—
33. Gold 23 car. 3¾ grs. -	140,10	136,30	3,80
34. Gold and copper in equal parts - - -	103,40	101,80	1,60
35. Gold with silver - -	153,40	153,40	—
36. Gold reduced by copper to 18 carats - -	121,80	121,80	—
37. Gold with silver -	153,40	151	2,40
38. Gold and copper in equal parts - - -	101,80	101,60	0,20
39. Gold with silver -	152,10	152,10	—
40. Gold with silver and copper	137,50	137,50	—
41. Gold with silver and copper	139,90	139,90	—
42 Gold with iron and copper	135,90	135.80	0,10
43. Gold with copper - -	136,90	125,70	11,20
44. Gold and copper in equal parts - - -	103,10	99,90	3,20
45. Gold with copper -	140	140	—
46. Gold with tin and copper	138	138	—
47. Gold with copper, cast in sand - - -	132,80	126,30	6,50
48. Gold with iron and copper	133,80	129,20	4,60
49. Gold with copper, cast in sand - - -	136	136	—
50. Gold of 18 carats -	125,30	125,30	—

* In this instance, there was an increase of weight, amounting to 0,40.

Table III. (*continued.*)

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
51. Gold with iron and copper	136,50	129,30	7,20
52. Gold with tin and copper	136,70	129	7,70
53. Gold with tin and copper	139,20	133,40	5,80
54. Gold of 18 carats - -	125,30	125,50*	—

The different pieces mentioned in the foregoing Table, are enumerated according to the order of their arrangement in the apparatus; and it is proper to observe, that the pieces of copper, Nos. 13 and 14, as well as the piece of gold made standard by copper, No. 17, and the piece of copper No. 18, were worn so thin during the experiment, that they were taken out after 114520 revolutions; but all the other pieces sustained twice that number, or 229040 revolutions.

When the preceding experiment was terminated, it was observed, that,

No. 15, or standard silver, was coated slightly by the copper No. 16.

That, No. 19, or gold made standard by copper, was coated by the standard silver No. 20.

That, No. 28, or standard silver, was slightly coated by the gold of 23 car. $3\frac{3}{4}$ grs. or No. 27.

That, part of No. 31, or gold of 23 car. $3\frac{3}{4}$ grs. adhered to the gold made standard by copper, or No. 32; and, that the gold alloyed by an equal proportion of copper, No. 38, was slightly coated by the gold made standard by silver, No. 37. But, to avoid repetition, it appears proper that other observations should be deferred, until the following experiment has been described.

* Increased in weight 0.20.

Experiment IV.

This resembled the former experiment, excepting that stamped pieces were employed.

Table IV.

Number of revolutions, 83520.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
1. Gold 23 car. $3\frac{3}{4}$ grs. -	142,10	142,50*	—
2. Gold 23 car. $3\frac{3}{4}$ grs. -	142,20	140,50	1,70
3. Gold with silver -	154,40	153,60	0,80
4 Gold with silver - -	159	158,90	0.10
5. Gold with silver and copper	137,60	137,40	0,20
6. Gold with silver and copper	137,70	136,80	0,90
7. Gold with copper -	135,50	135,30	0,20
8. Gold with copper - -	135,50	135,40	0,10
9. Gold with iron and copper	137,20	134 -	3,20
0. Gold with iron and copper	135,80	133,80	2,0
1. Standard silver -	85,80	84,50	1,30
2. Standard silver - -	86,20	84,50	1,70
3. Copper - - -	82,40	38	44,40
4 Copper - - -	82,10	46	36,10
5. Standard silver - -	85,40	82,90	2,50†
6. Copper - - -	85	82,40	2,60
7. Gold with copper -	140,30	117,10	23,20
8. Copper - - -	82,30	66,80	15,50
9. Gold with copper - -	134,50	134,40	0,10‡
10. Standard silver - -	85,20	83,20	1,90

* Increased in weight 0,40.

† The piece of silver was slightly coated by the copper.

‡ The standard gold was coated by the silver.

TABLE IV. (*continued.*)

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
21. Gold with copper -	139	138,70	0,30
22. Gold with silver - -	153,50	152,90	0,60
23. Gold with copper -	138,70	136,20	2,50*
24. Gold with iron and copper	138,50	137,10	1,40
25. Gold with copper -	142	142	—
26. Gold with silver and copper	137,80	137,80	—
27. Gold 23 car. 3¾ grs. -	140,10	137,80	2,30†
28. Standard silver - -	86,44	84,10	2,34
29. Gold 23 car. 3¾ grs. -	139,80	138,10	1,70
30. Gold with silver - -	156,80	155,40	1,40
31. Gold 23 car. 3¾ grs. -	140,80	137,60	3,20
32. Gold with copper -	141 60	144,20‡	—
33. Gold 23 car. 3¾ grs. -	141	140,10	0,90
34. Gold and copper in equal parts - - -	103,38	103,40§	—
35. Gold with silver - -	154,50	153,40	1,10
36. Gold of 18 carats -	121,02	121,80‖	—
37. Gold with silver -	155,50	153,40	2,10
38. Gold and copper in equal parts - - -	102	101,80	0,20¶
39. Gold with silver -	153,60	152,10	1,50
40. Gold with silver and copper	140	137,50	1,60

* The gold alloyed with copper was coated by the gold alloyed with iron and copper.

† The fine gold was coated by the silver.

‡ The standard gold was coated by the fine gold, and had gained 2,60.

§ The gold alloyed with an equal proportion of copper was coated by the fine gold, and had gained [illegible].

‖ The gold of 18 carats was coated by the gold alloyed with silver, and had gained 0,78.

¶ The gold alloyed with an equal part of copper was coated by the gold alloyed with silver.

TABLE IV. (*continued.*)

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
41. Gold with silver and copper	141	139.90	1,10 *
42. Gold with iron and copper	137.50	135,90	1.60
43. Gold with copper -	138,40	136.90	1,50
44. Gold and copper in equal parts - - -	103,14	103,10	,04 †
45. Gold with copper -	141	140	1,0 ‡
46. Gold with tin and copper	144,50	138	6.50
47. Gold with copper, cast in sand - - -	135,40	132,80	2,60
48. Gold with iron and copper	135,80	133,80	2,0
49. Gold with copper, cast in sand - - -	136,03	136	0,03
50. Gold of 18 carats -	125,38	125,30	0,08
51. Gold with iron and copper	137,40	136,50	0,90
52. Gold with tin and copper	141,80	136,70	5,10
53. Gold with tin and copper	140,90	139,20	1,70
54. Gold of 18 carats - -	125,02	125,30 §	—

* The gold alloyed with silver and copper was coated by the gold alloyed with iron and copper.

† The gold alloyed with an equal part of copper was coated by the gold made standard with copper.

‡ The gold alloyed with copper was coated by the gold alloyed with tin and copper.

§ Increased in weight 0,28.

The above effects sufficiently show, that the more ductile metals are always worn by those which are comparatively harder; and, in every experiment, it was constantly observed that the latter became coated by the metal of the former. This coating was commonly spread thinly over the surface; but, in some few instances, (especially when a very hard metal rubbed against one which was very soft,) the particles of the latter, instead of being spread over the whole surface, became accumulated, so as to form little protuberances or knobs.

It has been already observed, that the foregoing experiment was similar to that which preceded it, in respect to the quality, number, and arrangement of the pieces; and the only difference was, that the pieces employed in the present experiment were stamped with the die formerly mentioned.

As the continuance of the friction was not so long as that of the former experiment, it was not found necessary to remove any of the pieces, so that the complete series remained in the apparatus, during the whole of the experiment.

It will now be proper to compare the results of these two last experiments; and, in order to do this with more perspicuity, the following comparative Table, and some observations upon it, have been added.

Comparative Table, V.

Number of revolutions, 229040.				Number of revolutions, 83520.			
Plain or unstamped pieces.				Stamped pieces.			
Quality.		Loss.		Quality.		Loss.	
		Grains.				Grains.	
Gold 23 car. 3¼ grs.	1	3,0	6,30	Gold 23 car. 3¼ grs.	1	—	1,70; gained ,40; 1,30
	2	3,30			2	1,70	
Gold made stand. by silver	3	—	0,10	Gold made stand. by silver	3	0,80	0,90
	4	0,10			4	0,10	
Gold with silver & copper	5	6,40	9,70*	Gold with silver & copper	5	0,20	1,10
	6	3,30			6	0,90	
Gold with copper	7	—	0,10	Gold with copper	7	0,20	0,30
	8	0,10			8	0,10	
Gold with iron and copper	9	17,20	31,20	Gold with iron and copper	9	3,20	5,20
	10	14,0			10	2,0	
Standard silver	11	0,10	0,20	Standard silver	11	1,30	3,0
	12	0,10			12	1,70	
Copper	13	50,10	88,80	Copper	13	44,40	80,50
	14	38,70			14	36,10	

* The considerable diminution of these pieces was caused by an accident, which has been mentioned in a former note.

Comparative Table V. (*continued.*)

Plain or unstamped pieces.			Stamped pieces.		
Quality.		Loss.	Quality.		Loss.
		Grains.			Grains.
Standard silver - -	15	14,90	Standard silver - -	15	2,50
Copper - - -	16	35,80	Copper - - -	16	2,60
Gold with copper - -	17	33,70	Gold with copper -	17	23,20
Copper - - -	18	43,30	Copper - - -	18	15,50
Gold with copper - -	19	5,20	Gold with copper - -	19	0,10
Standard silver - -	20	8,20	Standard silver - -	20	1,90
Gold with copper - -	21	0,10	Gold with copper - -	21	0,30
Gold with silver - -	22	0,10	Gold with silver - -	22	0,60
Gold with copper - -	23	3,30	Gold with copper - -	23	2,50
Gold with iron and copper -	24	7,10	Gold with iron and copper -	24	1,40
Gold with copper - -	25	0 10	Gold with copper - -	25	—
Gold with silver and copper	26	—	Gold with silver and copper -	26	—
Gold 23 car 3¾ grs. -	27	0,10	Gold 23 car. 3¾ grs. -	27	2,30
Standard silver - -	28	0,10	Standard silver - -	28	2,34
Gold 23 car 3¾ grs. - -	29	—	Gold 23 car. 3¾ grs. - -	29	1,70
Gold with silver - -	30	—	Gold with silver - -	30	1,40
Gold 23 car. 3¾ grs. -	31	0,50 gained.	Gold 23 car. 3¾ grs. -	31	3,20 gained.
Gold with copper - -	32	0,40	Gold with copper - -	32	2,60
Gold 23 car. 3¾ grs. -	33	3,80	Gold 23 car. 3¾ grs. -	33	0,90 gained.
Gold and copper in equal parts	34	1,60	Gold and copper in equal parts	34	,02
Gold with silver - -	35	—	Gold with silver - -	35	1,10 gained.
Gold reduced by copper to 18 car.	36	—	Gold of 18 carats - -	36	0,78
Gold with silver - -	37	2,40	Gold with silver - -	37	2,10
Gold and copper in equal parts	38	0,20	Gold and copper in equal parts	38	0,20
Gold with silver - -	39	—	Gold with silver - -	39	1,50
Gold with silver and copper -	40	—	Gold with silver and copper	40	1,60
Gold with silver and copper -	41	—	Gold with silver and copper -	41	1,10
Gold with iron and copper -	42	0,10	Gold with iron and copper -	42	1,60
Gold with copper - -	43	11,20	Gold with copper - -	43	1,50
Gold and copper in equal parts	44	3,20	Gold and copper in equal parts	44	,04
Gold with copper - -	45	—	Gold with copper - -	45	1,0
Gold with tin and copper -	46	—	Gold with tin and copper -	46	6,50
Gold with copper, cast in sand	47	6,50	Gold with copper, cast in sand	47	2,60
Gold with iron and copper -	48	4,60	Gold with iron and copper -	48	2,0
Gold with copper, cast in sand	49	—	Gold with copper, cast in sand	49	,03
Gold of 18 carats - -	50	—	Gold of 18 carats - -	50	,08
Gold with iron and copper -	51	7,20	Gold with iron and copper -	51	6,90
Gold with tin and copper -	52	7,70	Gold with tin and copper -	52	5,10
Gold with tin and copper -	53	5,60 gained.	Gold with tin and copper -	53	1,70 gained.
Gold of 18 carats - -	54	0,20	Gold of 18 carats - -	54	0.28

From this comparative Table it appears, that although the experiments were made with correct instruments, and with every possible precaution, yet perfect accuracy could not be attained, nor indeed expected; for, various minute and unavoidable circumstances contributed to produce very sensible effects; even a few particles, collected and retained between the pieces during the operation, frequently prevented the loss by friction from being correctly ascertained. Another cause of irregularity in the comparative wear of the pieces, arose from a small degree of unevenness in the level of many of the unstamped faces, which, although scarcely perceptible to the eye, became sufficiently apparent when friction commenced, and pointed out the necessity of relying only upon general results. It would not, therefore, be right to lay too great a stress upon very small and only occasional deviations in the results; and consequently the small difference of a few fractional parts do not merit attention; for the same reason, it would not be proper to form an opinion upon certain results, which, without any very apparent cause, seem to be in opposition to each other. The most candid and certain mode to be adopted, under these impediments, appears therefore to be, that of taking into consideration only such effects as were general, under every change of circumstance, and which were invariably more or less the same, however the mode of operation might be diversified. Upon this basis it may be concluded, that the preceding experiments prove,

1st. That fine gold, or of 23 car. 3½ grs. when exposed to friction against gold of an equal quality, under the pressure of a considerable weight, suffers a very notable loss; and, although various circumstances seemed to indicate that but little effect in

respect to abrasion, is produced under a less weight, yet it must be remembered, that the first case may occur.*

Moreover, by the late experiments it has been proved, that fine gold, under all circumstances, is more subject to have any embossed or raised parts of its surface obliterated than any variety of alloyed gold; not always, nor indeed so much, by actual abrasion, as by having the protuberant parts pressed and rubbed into the mass, in consequence of its extreme softness and ductility.†

2dly. That fine gold, or of 23 car. $3\frac{3}{4}$ grs. when rubbed against the various kinds of alloyed gold, always or generally suffers the greatest comparative loss.

3dly. That gold reduced to 22 carats, or to standard, by silver, or by silver and copper, or merely by copper, suffers by friction, under general and similar circumstances, a smaller diminution than the fine gold abovementioned; and, with or without abrasion, the protuberant parts on the surfaces of these pieces remain

* It is proper to remark, that the preceding experiments were made under a much greater weight than can be supposed to operate generally during the circulation of money; and as, by some previous experiments, a less weight was found to produce, during a certain time, little or no effect, it may be suspected, that although, under a great pressure, fine or very ductile gold sustains a greater loss than some of those which are reduced to standard, yet, under a less pressure, or such as that which most commonly prevails in the course of the usual wear of coin, the reverse may probably be the case; for then the same causes operate with less rapidity, during a long period of time. From many various circumstances, there is reason therefore to believe, that the wear of coin against coin of a similar quality is, under a small or very moderate weight, in the inverse ratio to the degree of ductility; but this is only to be understood in the abovementioned case, of coin rubbed against coin of equal quality.

† This is, however, of much consequence; for, although coin may not suffer by actual abrasion, yet, if the impression made upon it can so soon be destroyed, it follows of course, that the pieces become (although still allowed to be current) no better than mere blanks, or fragments of a bar or ingot.

much more permanent, under all circumstances, than those of the fine gold. The difference of wear between the three kinds of standard gold abovementioned, does not in reality appear to be very considerable; but, upon the whole, the preference may be given to gold alloyed with a mixture of silver and copper, or to that which has only copper for the alloy.

4thly. That gold made standard partly by the addition of iron or tin, sustains a greater loss by friction than either of the three kinds of standard gold abovementioned.

5thly. That gold reduced to 18 carats by copper, is more liable occasionally to wear, in a small degree, than the three kinds of standard gold which have been lately mentioned, provided that the friction takes place between pieces of equal quality; but, in the contrary case, the principal loss always falls on the soft or standard gold, when it is opposed to gold of 18 carats, which is considerably harder.

6thly. That gold more debased than that of 18 carats, such as gold alloyed with an equal proportion of copper, suffers very considerably more than any of the kinds hitherto mentioned, provided that the pieces are of the same quality; but, on the contrary, fine and standard gold experience a very great loss, when exposed to the action of this debased gold, while the loss of the latter is comparatively much less.

7thly. That the wear of standard silver appears to be nearly equal with that of fine gold; but more than that of gold made standard by silver or by copper, and less than that of gold much debased by copper.

8thly. That, as gold which is not inferior to standard wears in general less than standard silver, so does this last suffer much [illegible] than copper.

The loss sustained by copper, when rubbed against copper, is infinitely more than that of the former metals; and, when these are exposed to the action of copper, they, as well as the copper, suffer a very considerable loss. This appears from the general results of these experiments, which prove, that pieces of metal which are the most subject to wear, are those which produce the greatest loss upon other pieces of metal, when rubbed against them; and it is remarkable, that *in such a case*, the loss does not always fall on one in preference to the other; so that the wear can only be considered in the aggregate, although one of the pieces may be regarded as the principal cause.

In order, however, to illustrate the results of the preceding experiments, as far as they concern the softer and harder kinds of standard gold, and to ascertain more fully the comparative wear of flat and smooth surfaces with that of such as were partly protuberant, the following experiment was made.

Experiment V.

In this experiment, two kinds of standard gold were employed, *viz.*

1st. Gold made standard by fine Swedish copper, which was very ductile; and,

2dly. Gold made standard by a mixture of fine Swedish copper and dollar copper. This was as brittle as was compatible with rolling and stamping; and was prepared by melting gold made standard by fine Swedish copper, with an equal quantity of gold rendered brittle by the standard proportion of Swedish dollar copper, which was mentioned in the first section of this Paper.

It may here be observed, that a distinction must be made

between hard and brittle metal. If a metal is disposed to crack when rolled, without requiring any extraordinary force to enable it to pass the rollers, then it may be regarded as brittle; but, if it requires an extraordinary force to make it pass the rollers, and is not disposed to crack, then it may be considered as hard.

The latter quality, or hardness, appears however in some degree to be produced, when a very brittle metal is gradually rendered ductile; at least it is difficult to distinguish a certain degree of hardness from a certain degree of brittleness, when the extremes of ductility and brittleness are nearly in equilibrio; and this was found to be the case, when gold was required to be made only so brittle as still to be capable of being rolled and stamped.

Some of the Swedish copper dollars were found to make gold very brittle, when employed as the alloy in standard proportion; but then this extreme brittle quality was incompatible with rolling and stamping. The standard gold, therefore, which was thus become so very brittle, was mixed with different proportions of very ductile standard gold, which had been alloyed with fine Swedish copper; and, after several trials, it appeared, that a mixture of equal parts of the very brittle standard gold and of that which was ductile, formed a metal the best adapted to the present purpose, as it then remained but just sufficiently ductile to be rolled and stamped.

TABLE VI.

Number of revolutions, 220000.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains
1. Gold made standard by fine Swedish copper, 4 pieces, unstamped - -	487,90	487.90	—
2. Gold made standard by fine Swedish copper, 4 pieces, stamped - - -	486,30	484	2,30
3. Gold made standard by equal parts of fine Swedish and dollar copper, 4 pieces, unstamped - -	564,70	564,70	—
4. Gold made standard as above, 4 pieces, stamped	564,30	550,40	13,90
5. Gold with fine copper, 2 pieces, unstamped -	244	243,90	0,10
Gold with fine and dollar copper, 2 pieces, unstamped	285,50	285,50	—
6. Gold with fine copper, 2 pieces, stamped - -	239,50	233,60	5,90
Gold with fine and dollar copper, 2 pieces, stamped -	279,90	276,50	3,40

This experiment proves,

1st. That very ductile standard gold, when exposed to the friction of gold of a similar quality, suffers less by abrasion than gold which is comparatively brittle, or harder, and which is subjected to friction under the same circumstances.

2dly. That when soft gold and brittle or hard gold rub against

each other, the greatest loss is sustained by the soft gold. And,

3dly. That pieces which have raised or embossed surfaces, suffer a greater loss, under every circumstance, than those which are smooth and flat.

The whole of the foregoing experiments were made with the machine called No. 1; and, as the friction was continued, in each experiment, during many days, with a pressure upon each couple of pieces equal to 3 lbs. 8 oz. 12 dts. and 21 grs., and as (considering the severity of such a trial) the loss sustained by the pieces, separately or collectively, was not very considerable, it may with reason be inferred, that standard gold does not easily suffer abrasion by the friction of metal against metal, or of coin against coin, especially under the circumstances which commonly prevail during the circulation of money.

In the machine No. 1, the pieces of gold were opposed face to face; it now therefore appeared proper, that the facts thus ascertained concerning the wear of gold, of different degrees of ductility, should be farther examined, and corroborated by a different method. To effect this, the second of the machines before described, which I shall call No. 2, was employed.

It has been already observed, that this machine was a cubic box, of 8 inches withinside, formed of oak one inch in thickness, through which, a strong axis of iron passed, so as to be turned by a wheel and pinion.

Experiment with the Machine No. 2.

Two hundred pieces of gold, of five different qualities, were employed in this experiment; and it must be previously remarked,

that twenty pieces of each kind of gold were plain and smooth, but that the others were stamped with the die which has several times been mentioned. The two hundred pieces were mingled, and were inclosed within the cubic box.

The following were the qualities of the gold,

1. Gold of 23 car. $3\frac{3}{4}$ grs.
2. Gold made standard by silver.
3. Gold made standard by silver and copper.
4. Gold made standard by fine Swedish copper.
5. Gold made standard by equal parts of fine Swedish copper and dollar copper.

TABLE VII.

Number of revolutions, 71720.

Quality.	Weight before friction.	Weight after friction	Loss.
	Grains.	Grains.	Grains.
1. Gold 23 car. $3\frac{3}{4}$ grs. 20 pieces, unstamped -	2716,8	2624	92,8
Gold 23 car. $3\frac{3}{4}$ grs. 20 pieces, stamped - - -	2691,4	2595,8	95,6
2. Gold made standard by silver, 20 pieces, unstamped	2719	2655,5	63,5
Gold made standard by silver, 20 pieces; stamped -	2722,6	2662,5	60,1
3. Gold made standard by silver and copper, 20 pieces, unstamped - -	2720	2708	12,
Gold made standard by silver and copper, 20 pieces, stamped - - -	2724,7	2713	11,7

TABLE VII. (continued.)

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
4. Gold made standard by fine Swedish copper, 20 pieces, unstamped - -	2746	2728	18
Gold made standard by fine Swedish copper, 20 pieces, stamped - -	2738,1	2718,9	19,2
5. Gold made standard by equal parts of fine and dollar copper, 20 pieces, unstamped - - -	2799,5	2786,5	13
Gold made standard by equal parts of fine and dollar copper, 20 pieces, stamped	2802,7	2790,6	12,1

	Grains.
Total weight of the unstamped pieces, before friction,	13701,3
Total weight of the stamped pieces, before friction -	13679,5
Total loss of the unstamped pieces - - - -	199,3
Total loss of the stamped pieces - - - -	198,7

The 71720 revolutions of the preceding experiment, were performed in 40 hours; after which, the pieces were taken out, as those parts of the hollow cube which were the most exposed to friction, were nearly half worn through.

All the pieces appeared to have suffered more on the edges than on the faces; and those which were stamped had the impression more or less obliterated or flattened, in proportion to their respective degree of ductility, or to the loss which,

according to the result of this experiment, they had relatively sustained.

The different pieces, after the experiment, had a curious appearance; for, on the edges, which were become round and polished, a small regular raised bead or moulding was formed, which surrounded each face, like a frame; and both faces were become more or less concave.

The original diameter of the pieces was also diminished, nearly according to their different degrees of ductility, and according to the loss which they had experienced in consequence of the operation.

The measure of the diameters of the pieces, after the experiment, was,

Gold 23 car. $3\frac{3}{4}$ grs. eight-tenths of an inch and $\frac{3}{40}$.

Gold alloyed with silver, nine-tenths of an inch.

The others varied little from nine-tenths and $\frac{1}{40}$; which was less, by about $\frac{1}{40}$ of an inch, than the original diameter of the pieces; and it was evident, that the pieces of fine gold and those consisting of gold alloyed with silver, being the most ductile of the whole series, had suffered the greatest loss, and also that they were those which became the most diminished in diameter. Upon the whole, therefore, considering the general result of this experiment, it appears to corroborate what has been asserted concerning the former experiments, *viz.* that soft or ductile gold suffers the greatest loss, when exposed to friction in contact with gold which is comparatively harder. The effects upon gold of 23 car. $3\frac{3}{4}$ grs. and upon gold alloyed with silver, fully prove this; and, if a perceptible difference was not found between the others, in this experiment, it must be ascribed to the difference in ductility being too small to resist the general effect of the friction;

and, allowing that to be the case, such a difference cannot be deemed worthy of notice.

Before the observations upon the foregoing experiment are concluded, it may be proper to add, that no essential difference between plain and stamped pieces could be observed, when friction was applied in the way abovementioned.

Here terminated the experiments which were intended to ascertain the effects arising from the friction of coin against coin; but it will probably be better to postpone the general observations upon this part, until the experiments have been described which were made with the machine Fig. 3.

By this apparatus, which may be called No. 3, various pieces were exposed to the action of certain powders and filings of metals, which were separately sprinkled upon the horizontal table.

The pieces were properly fixed in their respective sockets and frames, and were placed so as to bear upon the table, with or without additional weights.

The table was moved by a wheel and pinion, so calculated as to avoid too rapid a motion; and the revolutions were denoted, as in the former experiments, by means of a counter.

Experiments made with the Machine No. 3.

In the first experiment which was made with this instrument, the table was covered with fine powdered whiting, and the pieces were arranged as follows*.

* Two pieces of each of the different kinds of gold, &c. were subjected to this experiment.

Experiment I.

Table VIII.

Number of revolutions, 11880.

Quality.	Weight before friction.	Weight after friction	Loss.
	Grains.	Grains	Grains.
Gold 23 car. $3\frac{3}{4}$ grs. unstamped	284,20	283,40	0,80
Gold 23 car. $3\frac{3}{4}$ grs. stamped	283,90	280,10	3,80
Gold made standard by silver, unstamped - - -	293	292,30	0,70
Gold made standard by silver, stamped - - -	289,70	287	2,70
Gold made standard by silver and copper, unstamped -	280,80	280,50	0,30
Gold made standard by silver and copper, stamped -	282,20	280,40	1,80
Gold made standard by fine copper, unstamped -	245,70	245	0,70
Gold made standard by fine copper, stamped - -	242,50	240,60	1,90
Gold made standard by equal parts of fine and dollar copper, unstamped - -	245,30	245,10	0,20
Gold made standard by equal parts of fine and dollar copper, stamped - -	248,90	247,70	1,20
Gold of 18 carats,* unstamped	209,70	209,50	0,20
Gold of 18 carats, stamped -	207,30	206,50	0,80
Standard silver, unstamped -	144,20	143,20	1
Standard silver, stamped -	145,40	143,60	1,80
Fine copper, unstamped -	151	150,80	0,20
Fine copper, stamped - -	154,60	154,40	0,20

* The gold of 18 carats employed in these experiments, was alloyed with copper; but I am well convinced that gold reduced to 18 carats by an alloy composed of silver and copper in different proportions, will be more easily worked than when copper alone forms the alloy, and will, in many respects, be found very useful by goldsmiths and jewellers.

From the result of this experiment it appears, that by the action of a soft powder, such as whiting, fine gold sustained a greater loss than gold made standard by silver; and again, that this, being more ductile than any of the other kinds of standard gold, suffered more than those; for it is evident, that the wear produced by this experiment, was in proportion to the softness or ductility of the pieces of metal, those which were comparatively hard, being in general those which were the least abraded.

In the same order also, the difference between plain and stamped surfaces was perceptible.

It must be likewise remarked, that although copper, when rubbed against copper, experiences a much greater loss than either gold or silver, yet, when copper is exposed to the action of a powder like whiting, as in the present experiment, it is, on the contrary, the metal which is abraded in the smallest degree.

Lastly, it may be observed, that this experiment fully proves, that the wear is much greater upon raised or embossed surfaces than upon those which are flat and plain; and that, in proportion to the ductility of the metal, the difference of wear between plain and stamped pieces becomes more apparent.

The preceding experiment was made with a weight upon each piece, equal to 3 lbs. 8 oz. 12 dts. and 21 grs. which was the same as was employed in every experiment made with the machine No. 1. But, previous to the following experiment, in which fine white writing-sand was used instead of whiting, it was found, after 2480 revolutions, that no inference could be made when this weight was employed; for the sand soon began to accumulate upon the faces of the pieces, and adhered like a dark gray or blackish crust, which with great difficulty was detached. In consequence of this, the wear of the pieces

was extremely unequal; and, towards the conclusion of the experiment, they were so coated by the abovementioned crust, as no longer to be abraded; and the horizontal wooden table alone experienced the effects of the friction.

After various trials, it was found necessary to remove the leaden weights, = 19825 grains, which had been hitherto always employed. So that the pieces in the experiment now to be related, were only pressed upon the table by the weight of their respective brass frames, = 1604 grains; and, in addition to other precautions in this experiment, the sand was not loosely scattered, but was cemented upon the horizontal table by a solution of isinglass.

By these means, the following experiment was made with success.*

Experiment II.

Table IX.

Number of revolutions, 880.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Gold 23 car. $3\frac{3}{4}$ grs. unstamped	286,70	268,60	18.10
Gold 23 car. $3\frac{3}{4}$ grs. stamped	288,70	286,60	20,10
Gold made standard by silver, unstamped - - -	271,30	254,80	16,50
Gold made standard by silver, stamped - - -	271,40	253,80	17,60

* Two pieces of each of the various kinds of gold, &c. were subjected to this experiment.

TABLE IX. *(continued.)*

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Gold made standard by silver and copper, unstamped -	282,70	271,10	11,60
Gold made standard by silver and copper, stamped -	282,20	267,20	15
Gold made standard by fine copper, unstamped -	243,70	228,20	15,50
Gold made standard by fine copper, stamped - -	243,60	229,70	13,90
Gold made standard by equal parts of fine and dollar copper, unstamped - -	249,70	237,80	11,90
Gold made standard by equal parts of fine and dollar copper, stamped - -	249,60	234,80	14,80
Gold of 18 carats, unstamped	208,90	199,30	9
Gold of 18 carats, stamped -	209	197,10	11,90
Standard silver, unstamped -	144,60	134,80	9,80
Standard silver, stamped -	144,80	134,40	10,40
Fine copper, unstamped -	151,60	144,40	7,20
Fine copper, stamped - -	163,80	154,20	9,60

It appears unnecessary to make any observations upon this experiment, as the results of it so nearly correspond with those of the former, in which whiting was employed.

Experiment III.

Two pieces of each kind of gold, &c. were fixed as before; and the horizontal table was covered with filings of gold made standard by copper. The filings were fixed with a solution of

isinglass; and, after several trials, it was found necessary to replace the weights, which had been removed when the preceding experiment was made, each separate weight being equal to 3 lbs. 8 oz. 12 dts. and 21 grs.

Table X.

Number of revolutions, 660.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Gold 23 car. 3¾ grs. unstamped	288	278,30	9,70
Gold 23 car. 3¾ grs. stamped	290,20	276,40	13,80
Gold made standard by silver, unstamped - -	268,80	265,70	3,10
Gold made standard by silver, stamped - - -	270,20	264,90	5,30
Gold made standard by silver and copper, unstamped -	278,90	278,80	0,10
Gold made standard by silver and copper, stamped -	281,70	281,30	0,40
Gold made standard by fine copper, unstamped -	239,80	239,20	0,60
Gold made standard by fine copper, stamped - -	261,60	261,20	0,40
Gold made standard by equal parts of fine and dollar copper, unstamped - -	247,70	247,30	0,40
Gold made standard by equal parts of fine and dollar copper, stamped - -	246,40	245,90	[illegible]
Gold of 18 carats, unstamped	208,80	208,70	0,10
Gold of 18 carats, stamped -	207,90	207,70	0,20

TABLE X. *(continued.)*

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Standard silver, unstamped -	144,80	144	0,80
Standard silver, stamped -	147	146,20	0,80
Fine copper, unstamped -	164	164	—
Fine copper, stamped -	146,60	146,40	0,20

It would be superfluous to make any remarks upon this experiment, as the general results of it agree with those made with the same apparatus, which have been lately described.

Mr. CAVENDISH observes, that if those cases in Table III. are selected, in which one of the pieces rubbed was gold made standard by copper, then, the effect will be discerned which was produced by the friction of different kinds of gold against that metal in a solid form: and again, in Table X. may be seen the effect which was produced by the friction of different kinds of gold, &c. against the same metal, in the form of filings.

TABLE XI.

Quality of the metal.	Table III. Loss per piece.	Table X. Loss per piece.
Fine gold - - - -	0,50	4,85
Gold with silver - - -	0,10	1,55
——— silver and copper -	—	0,05
——— copper - - -	0,05	0,30
Gold of 18 carats - - -	—	0,05
Standard silver - - -	8,20	0,40
Copper - - - - -	48,30	—

The whole motion of the pieces of coin, in the experiment of Table III. was about 687000 inches; while the whole motion of the pieces in the experiment of Table X. was only 62205. The weight by which the pieces were pressed was the same in both experiments, so that the pieces sustained eleven times more friction in the former than in the latter experiment. But it is worthy of notice, that the greater part of the different metals suffered the most considerable diminution by the last mode of examination; and this may be regarded as an additional instance of the small diminution which metal suffers by being rubbed against metal in a solid form, considered comparatively with that which it suffers when rubbed against the same metal in small particles.

Moreover, it is remarkable, that although the diminution of all the metals which have been examined, excepting silver and copper, is less in the IIId than in the Xth Table, yet that of copper is many times greater.

Mr. Cavendish, considering how necessary it was to determine whether there is any material difference, in point of wear, between gold coin intirely of the same quality and that in which the gold is of different qualities, was induced to make the following Table of comparison, formed upon the results stated in Table V.

TABLE XII.

Quality of the metal.		Unstamped pieces. Loss per piece.				Stamped pieces. Loss per piece.		
		1st metal.	2d metal.	Opposed.	Mean.	1st metal.	2d metal.	Opposed.
1.	2.	A.	B.	C.	D.	A.	B.	C.
ne gold - -	Gold and silver	3,15	,05	—	—	,65	,45	1st metal. 1,70
ne gold -	Gold and copper	3,15	,05	1st metal. ,50	,05	,65	,15	1st metal. 3,20
old and silver	Gold and copper	,05	,05	,10	,10	,45	,15	1st metal. ,60
old, silver, and copper - -	Gold, iron, and copper -	uncer.	15,60	2d metal. ,10	,05	,55	2,60	2d metal. 1,60
old and copper -	Gold, iron, and copper -	,05	15,60	2d metal. 7,10	5,20	,15	2,60	1st metal. 2,50
old and silver - -	Gold, silver, and copper -	,05	uncer.	—	—	,45	,55	2d metal. 1,60
old, silver, and copper -	Gold and copper -	uncer.	,05	2d metal. ,10	—	,55	,15	—

The two first columns of this Table show the quality of the two metals compared. The columns A B show the loss which each of these metals suffered, when rubbed against metal of the same kind; and the two next columns show their diminution, when rubbed against each other; the column C showing which metal wore the most, together with its diminution, and the column D showing the mean between the diminutions of the two metals. For example, in the first row of the Table, the two metals compared are fine gold and gold alloyed with silver; and, when stamped pieces were employed, the diminution caused by rubbing the first metal against metal of the same kind was 0,65, and the loss of the second metal, by the same treatment, was 0,45; but, when the two metals were rubbed against each other, the first metal was most diminished, and its loss was 1,70; therefore, as the second metal lost 1,40, the mean loss was 1,55.

To judge from the unstamped pieces, it should seem as if a variation in the quality of the gold tended rather to diminish the wear; but, from the effects of the stamped pieces, which are more to be regarded, it seems otherwise. Upon the whole, however, it does not appear to be a matter of much consequence, that the coin should consist intirely of gold of exactly the same quality.

In the following experiment, filings of iron were employed, and were fixed upon the horizontal table by a solution of isinglass. The leaden weights, and the general arrangement, remained as before.

Experiment IV.

TABLE XIII.

Number of revolutions, 404.

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Gold 23 car. $3\frac{3}{4}$ grs. unstamped	276,40	251,40	25
Gold 23 car. $3\frac{3}{4}$ grs. stamped	278,30	251,10	27,20
Gold made standard by silver, unstamped - -	264,90	244,90	20
Gold made standard by silver, stamped - -	265,70	239,40	26,30
Gold made standard by silver and copper, unstamped -	281,30	258,50	22,80
Gold made standard by silver and copper, stamped -	278,80	252,70	26,10
Gold made standard by fine copper, unstamped -	261,20	240,90	20,30
Gold made standard by fine copper, stamped - -	259,20	212,30	26,90

TABLE XIII. *(continued.)*

Quality.	Weight before friction.	Weight after friction.	Loss.
	Grains.	Grains.	Grains.
Gold made standard by equal parts of fine and dollar copper, unstamped - -	245,90	218,10	27,80
Gold made standard by equal parts of fine and dollar copper, stamped - -	247,30	219,90	27,40
Gold of 18 carats, unstamped	207,70	197,50	10,20
Gold of 18 carats, stamped -	208,70	197,80	10,90
Standard silver, unstamped -	146,20	131,80	1[illegible],40
Standard silver, stamped -	144	127,60	16,40
Fine copper, unstamped -	146,40	133,80	12,60
Fine copper, stamped -	164	151	13

This experiment proves, that the difference of wear, thus produced upon pieces of gold which do not differ very considerably in comparative ductility, cannot easily be discovered, when the material or method employed to produce abrasion acts with violence and rapidity.

For example, the difference of wear between fine gold and the various kinds of standard gold, appeared more evidently defined, when a soft powder like whiting was employed, and when the abrasion was very slow and gradual, than by the subsequent experiment with sand; but, even in this case, the different and progressive effects [illegible] sufficiently apparent. On the contrary, in this last experiment, with the iron filings, very little difference could be perceived, between the wear of fine gold and that of the various kinds of standard gold; for, the rapid and

violent action of the iron filings, resembling that of a rasp, was too powerful to be modified in any very perceptible manner by the different ductility of these pieces; and, consequently, little or no variation in the wear could be observed, excepting in the pieces of gold reduced to 18 carats by copper, which, being hard when compared with the former, were in some measure better able to resist the effects of the filings.

The following comparative Table of the four preceding experiments, will show, by the near agreement of their general results, that the wear of gold, when exposed to the friction of earthy powders, or metallic filings, is in proportion to the relative degree of ductility.

Table XIV.

Comparative Table of the Four preceding Experiments.

	Experiment I.		Experiment II.		Experiment III.		Experiment IV.	
	Revolutions. 11880. Whiting.		Revolutions. 880. Sand.		Revolutions. 660. Filings of Standard gold.		Revolutions. 404. Filings of Iron.	
Quality of the pieces of gold.	Loss.		Loss.		Loss.		Loss.	
	Grains.		Grains.		Grains.		Grains.	
Gold 23 car. 3½ grs. - -	0,80 3,80	4,60	18,10 20,10	38,20	9,70 13,80	23,50	25 27,20	52,20
Gold made standard by silver	0,70 2,70	3,40	16,50 17,60	34,10	3,10 5,30	8,40	20 26,30	46,30
Gold made standard by silver and copper - -	0,30 1,80	2,10	11,60 15	26,60	0,10 0,40	0,50	22,80 26,10	48,90
Gold made standard by fine copper - -	0,70 1,90	[illegible]	[illegible] [illegible]	[illegible]	0,60 0,40	1	[illegible] [illegible]	47,20

TABLE XIV. *(continued.)*

	Experiment I.		Experiment II.		Experiment III.		Experiment IV.	
	Revolutions. 11880. Whiting.		Revolutions. 880. Sand.		Revolutions. 660. Filings of Standard gold.		Revolutions, 404. Filings of Iron.	
Quality of the pieces of gold.*	Loss.		Loss.		Loss.		Loss.	
	Grains.		Grains.		Grains.		Grains.	
Gold made standard by equal parts of fine and dollar copper	0,20 1,20	1,40	11,90 14,80	26,70	0,40 0,50	0,90	27,80 27,40	55,20
Gold of 18 carats	0,20 0,80	1	9 11,90	20,90	0,10 0,20	0,30	10,20 10,90	21,10
Standard silver	1 0,80	2,80	9,80 10,40	20,20	0,80 0,80	1,60	11,40 16,40	27,80
Fine copper -	0,20 0,20	0,40	7,20 9,60	16,80	— 0,20	0,20	12,60 13	25,60

When the whole of the preceding experiments, made with the three different machines, are viewed and compared, their general results appear to be as follows.

1st. That when equal friction, assisted by a moderate pressure, takes place between pieces of coin which are in each series of a similar quality, then, abrasion is most commonly produced in an inverse ratio to the degree of ductility.

2dly. That the contrary effect happens, when pieces of different qualities rub against each other; for then, the more ductile metal is worn by that which is harder.*

* [illegible] upon pure and upon alloyed silver, the [illegible] those of the present experiments upon gold. [illegible] mentioned as follows. "L'objection la plus [illegible] dans les monnoies, est la crainte [illegible] ne s'usent plus vite. [illegible] que l'alliage leur [illegible]

3dly. That earthy powders and metallic filings produce similar effects, and tend to wear the different kinds of gold in proportion to their respective degrees of ductility.

Fine gold, being extremely soft and ductile, sustains a considerable loss, under many of the general circumstances of friction; and as at all times it appears certain, that the impressions which have been stamped upon it are most easily obliterated, even when actual abrasion does not take place, there is much reason to conclude, that gold of such extreme ductility is not that which is the most proper to be formed into coin.

But gold of the opposite quality, or at least so hard as to be

"augmente-t'elle ou diminue-t'elle la perte qu'elles essuient par le frottement? C'est "une question qui n'a jamais été résolue par des expériences directes; et l'Academie "se propose d'en faire, pour éclairer un fait dont la connoissance peut être utile, non "seulement pour l'art de fabriquer les monnoies, mais pour un grand nombre d'autres. "*Les premières expériences ont prouvé, que les monnoies d'argent pur perdoient moins* "*que les monnoies alliées, lorsque le frottement avoit lieu entre des pièces semblables,* "*mais qu'elles perdoient davantage, lorsque le frottement avoit lieu entre les pièces* "*pures et les pièces alliées.*"

Rapport fait a l'Academie des Sciences, le 27 Octobre, 1790, sur les titres des Metaux monnoiés, &c. par MM. Borda, Lagrange, Lavoisier, Tillet, et Condorcet. *Annales de Chimie,* 1793, Tome XVI. p. 230, et 231.

The effects thus stated to have been produced upon pure and upon alloyed silver, most probably in like manner prevail in respect to gold; but this cannot be stated as a certain fact; for, although there is much reason to suppose, that under a small or very moderate pressure, the wear of gold against gold of an equal quality is uniformly in an inverse ratio to the degree of ductility, and allowing that under such circumstances fine gold would suffer a less diminution than gold which is alloyed, yet the present experiments prove, that under a considerable pressure, the order of wear is in some measure different; for, extremely soft or fine gold is then found to suffer as great, or indeed a greater diminution, than gold which is but moderately ductile: and the whole of the experiments which have been made for the purpose of the present investigation, concur to show, that gold which is neither extremely soft nor extremely hard, is best adapted to resist friction in general.

just capable of being rolled and stamped, seems to be equally improper for the purpose of coin. For, even supposing that hard gold suffered, in every case, less by friction than that which is moderately ductile, (which is not however the fact,) and allowing that standard gold may, by a mixed alloy, be rendered as hard as gold reduced by copper to 18 carats, without changing the standard proportion of gold, yet it would be very difficult always to make such standard gold of an uniform degree of hardness. Moreover, by some experiments which I purposely made at the Mint, upon the rolling and stamping of gold of different qualities, it evidently appeared, that gold equal in hardness to that of 18 carats, could not be employed with advantage; for, the additional labour which was required for the rolling and stamping of this hard gold, the frequent failure in making the impression, and the battering and breaking of the dies, fully proved, that the expense and difficulty attending the working of such gold, would by no means be compensated by any small degree of durability which it might possess over any other.

The extremes of ductility and of hardness being therefore equally objectionable, it follows of course, that gold of moderate ductility must be that which is the best adapted for coin; and, as nothing but silver or copper can be employed to alloy gold which is intended to be coined, it may be here observed, that whatever might have been the original motive for introducing the present standard of 22 carats, yet it appears, from the experiments lately described, that this proportion of $\frac{1}{12}$, of the abovementioned metals, is (every circumstance being considered) the best, or at least as good as any, which could have been chosen.

There is, however, some difference in the quality of gold, when alloyed with the standard proportion of silver, of silver

and copper, and of copper, which requires to be considered.

Gold alloyed with one-twelfth of silver, is of a fine but pale yellow; it is very ductile; it is easily rolled, and may be stamped without being annealed; it consequently does not require to be blanched; and, after the complete process of coining, the surface and every part remains of an uniform quality, so that, by wear, it does not appear of different colours.

These properties are certainly much to be valued; but the objections to this kind of standard gold are,

1st. The additional expense attending the use of silver as an alloy.

2dly. The extreme pale yellow colour. And,

3dly. That, from its great ductility, it is almost as liable to have the impressions which have been made upon it obliterated, as those which have been made upon fine gold.

All things being therefore considered, gold alloyed only with silver, does not appear to be so proper for coin as may at first be imagined.

Gold made standard by a mixture of equal parts of silver and copper, is not so soft as gold alloyed only with silver; neither is it so pale, for it appears to be less removed from the colour of fine gold than either the former or the following metal.

Gold alloyed with silver and copper, when annealed, does not become black, but brown; and this colour is more easily removed by the blanching liquor, or solution of alum, than when the whole of the alloy consists of copper. It may also be rolled and stamped with great facility; and, under many circumstances, it appears to suffer less by friction, than gold alloyed by silver only, or by copper.

But, after it has been subjected to the ordinary friction which must take place during the circulation of money, it is liable to appear of a deeper colour in those parts which are prominent, and are consequently the most exposed to friction. This defect arises from a cause which will soon be explained, but it cannot be regarded as an objection of any weight.

The last kind of standard gold which remains to be mentioned, is that which is alloyed only by copper. This is of a much deeper colour than those which have been hitherto noticed, and it is slightly harder than either of them; but nevertheless it is very ductile, provided that the copper be pure. It requires to be annealed, and then becomes nearly or quite black; which colour is not so easily removed by the blanching liquor, as that which is produced by the process of annealing, upon gold alloyed with a mixture of silver and copper.

It suffers less by many of the varieties of friction, than gold which is alloyed with silver; but, in some cases, it seems to wear rather more than gold alloyed with silver and copper; the difference is not however very considerable.

This sort of standard gold, as well as that which is alloyed with silver and copper, appears commonly, after a certain degree of wear, of a coppery colour, more or less deep, in those parts which are the most prominent; and, when coin thus alloyed exhibits such an appearance, it is frequently and vulgarly said to have been in contact with copper money; and sometimes guineas having this appearance have been refused, upon the supposition that they were debased. But the real fact is, that when copper constitutes part or the whole of the alloy, it becomes oxidated or calcined upon the surface of the blanks, by the process of annealing; and the blackish crust of copper, in

this state, must then be removed by the solution of alum, called the blanching liquor. Now it is evident, that after this operation, the surfaces of the blanks or unstamped pieces, can no longer be regarded as standard gold. For, if copper alone forms the alloy, it must be dissolved and separated from the surface of each piece of coin; and the same effect must also take place, with respect to the copper, in the alloy formed of copper and silver. So that, in the first case, each piece, when blanched, will consist of gold made standard by copper, covered with a thin coat of fine gold; and, in the second case, each piece will be composed of gold made standard by silver and copper, coated with gold alloyed with $\frac{1}{24}$ of silver, or with half of the standard proportion of alloy, supposing the silver and copper to have been in equal quantities. As, therefore, the standard gold of which the pieces consist, is always, more or less, of a deeper colour than the coating or film of the finer gold which covers each piece, it must be evident, that when this coating has been rubbed and removed from the raised or prominent parts, these will appear of a very different and deeper colour than the flat part or ground of the coin. The reason therefore is sufficiently apparent, why gold which is alloyed with silver only, cannot be liable to this blemish.

Upon a comparison of the different qualities of the three kinds of standard gold which have been lately mentioned, it appears, (strictly speaking,) that gold made standard by silver and copper is rather to be preferred for coin; but, as gold made standard by copper alone is not very much inferior in its general properties, it may be questioned, whether the few advantages which are thus gained, will compensate the additional expense of the silver required for half of the alloy; and, indeed,

any extraordinary addition of silver appears to be the less necessary, as there is commonly some silver in the gold which is sent to the Mint, which, being reckoned as part of the alloy, contributes to produce those beneficial effects which result when silver is purposely added.

From a general view of the present experiments, there does not appear to be any very great or remarkable difference in the comparative wear of the three kinds of standard gold, all of which suffer abrasion slowly, and with much difficulty; and (as it has been already observed) the difference of wear between the two last mentioned, is certainly but inconsiderable. For these reasons, and from the consideration of every other circumstance, it must be evident, that the extraordinary loss which the gold coin of this kingdom is stated to have sustained within a certain limited time, cannot, with even a shadow of probability, be attributed to any important defect in the composition or quality of the standard gold; and all that can be said upon this subject is, that some portion of this loss may have been caused by the rough impression and milled edge now in use, by which, each piece of coin acts, and is acted upon by the others, in the manner of a file.

The loss thus occasioned cannot however be considerable; for the quality of the present standard gold is certainly that which is well adapted to resist abrasion, especially in the case of the friction of coin against coin; and this is strongly corroborated by the observations of bankers and others, who are in the habit of sending or receiving large quantities of gold coin from any considerable distance. When a number of guineas, rather loosely packed, have been long shaken together by the motion of a coach or other carriage, the effects of friction are observed

chiefly to fall upon only a few of the pieces. But it is not a little remarkable, that although these are often reduced nearly or quite to the state of plain pieces of metal or blanks, yet, upon being weighed, they are found to have sustained little or no loss; and from this it appears, that the impressions have been obliterated, not by an actual abrasion of the metal, but by the depression of the prominent parts, which have been forced into the mass, and become reduced to a level with the ground of the coin. Pieces of hard gold would not so easily suffer by depression; but the real loss would probably be greater, they being, in the case of the friction of coin against coin of similar quality, more susceptible of abrasion.

Upon the whole, there is every reason to believe, that our gold coin suffers but little by friction against itself; and the chief cause of natural and fair wear probably arises from extraneous and gritty particles, to the action of which the pieces may occasionally be exposed in the course of circulation. But still it must be repeated, that the united effects of every species of friction to which they may be subjected, *fairly and unavoidably*, during circulation, cannot produce any other wear than that which is extremely gradual and slow, and such as will by no means account for the great and rapid diminution which has been observed in the gold coin of this country.

As the general results of each part of this inquiry have been noticed at the close of the different sections, a regular recapitulation would be superfluous. We may however observe, that the experiments on the various alloys of standard gold, concur with established practice and opinion to prove, that only two of the metals, *viz.* silver and copper, are proper to be employed in

the reduction of fine gold to standard for the purpose of coin; but, at the same time, it must be allowed, that the phenomena exhibited by arsenic, antimony, zinc, cobalt, nickel, manganese, bismuth, lead, tin, iron, and platina, are in many respects remarkable, and demonstrate the utility of a regular experimental investigation of metallic alloys.

The contents of the second section show, that numerous causes influence the specific gravity of metals; and, amongst these, it has been long ago observed, that some metals, when added to others, generally produce contraction in the bulk of the mass, or an increase of specific gravity, but that other metals produce effects which are precisely the reverse. This, to a certain extent, is unquestionably true; but it does not follow that each metal which produces contraction or expansion, when added to others, should (as some have supposed) constantly produce similar effects, corresponding to the relative proportion of that particular metal; for we well know that some metals, in like manner, promote the fusibility of others in a much greater degree than could be expected from their natural or inherent fusible property; but then, the maximum of this effect is produced by certain proportions of the different metals, and suffers diminution by any variation in these proportions. It appears, therefore, that this great degree of fusibility, is a property peculiar to the compound, which cannot immediately be attributed to either of the component metals; and I am much deceived, if those alterations in specific gravity which, in like manner, are observed in the various metallic compounds, will not admit of a similar explanation; for, in respect to the contraction or expansion which takes place in consequence of combination, we [illegible] believe, that there is a maximum of this contraction or

expansion, dependent on a certain relative proportion of the different metals.

The experiments on the comparative wear of gold, which are described in the last section, were attended with considerable difficulties; for this reason, the conclusions have been founded only upon such facts as were uniformly the same under every circumstance. These general conclusions have been already fully stated; but we may again observe, that gold of moderate ductility is (all things being considered) the best adapted to the purpose of coin, and that the real wear of such coin is, in all probability, very slowly effected; so that a long period of time must elapse, before any considerable diminution in weight can be perceived.

The experiments contained in these three sections were limited to standard gold; and, allowing that some curious and instructive facts have been discovered, still more might have been expected from an extension of such experiments to gold variously alloyed in every possible proportion. But an immense addition to metallurgical science would, in all probability, be derived from a comparative investigation of the whole of the known metallic substances, formed into binary, ternary, and such like combinations, proceeding from the most simple to the most complicated, and accompanied by accurate observations on the lustre, colour, ductility, hardness, specific gravity, and fusibility of the compounds.

Our actual knowledge of the properties of metallic mixtures is certainly very imperfect, and has by no means kept pace with the rapid progress of modern chemistry. Few additions have been made to the compound metals employed by the ancients.

The various mixtures of gold and silver, called electrum,* those of the Corinthian metal,† the varieties of bronze,‡ the compound of copper and zinc now called brass,§ the metal for specula,|| the metal called argentarium,¶ (in some measure answering to our pewter,) the art of plating and of tinning,** and the process of amalgamation,†† evince how great a progress had been made by the ancients in the mixing and working of metals.

Much, however, remains to be done, and much may be expected, from a regular and systematical series of experiments on the properties of compound metals. For, exclusively of the immediate application of many of the alloys to economical purposes, it cannot be doubted that science will derive other considerable advantages; our ideas concerning the properties of the metals, whether simple or mixed, will be much enlarged, and clouds of errors, with the traditionary prejudices which as yet shade this branch of human knowledge, will be dispersed.

* PLINIUS, lib. XXXIII. cap. iv.

† PLIN. lib. XXXIV. cap. ii.

‡ Ibid. and cap. ix.

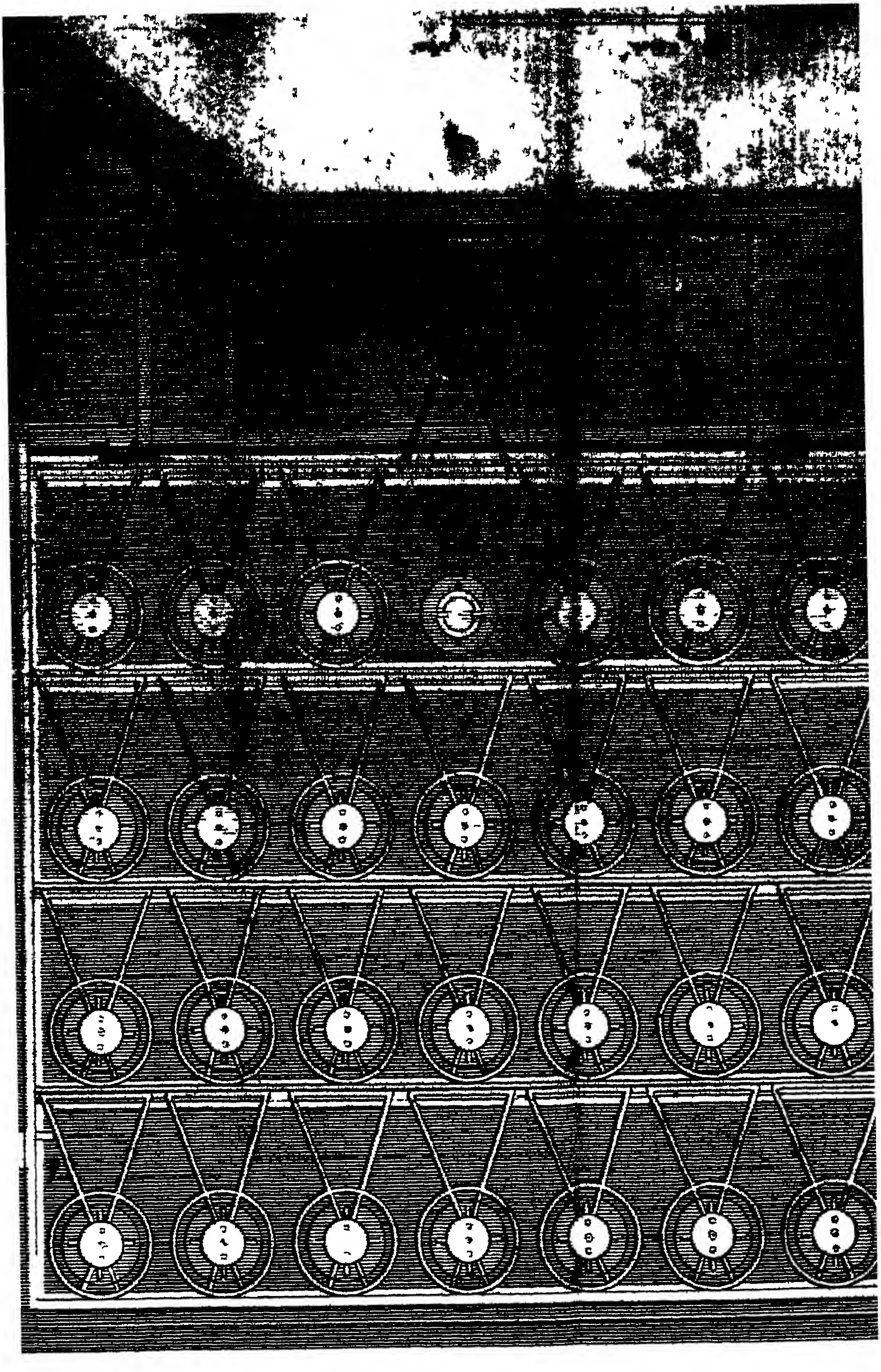
§ PLIN. lib. XXXIV. cap. x.

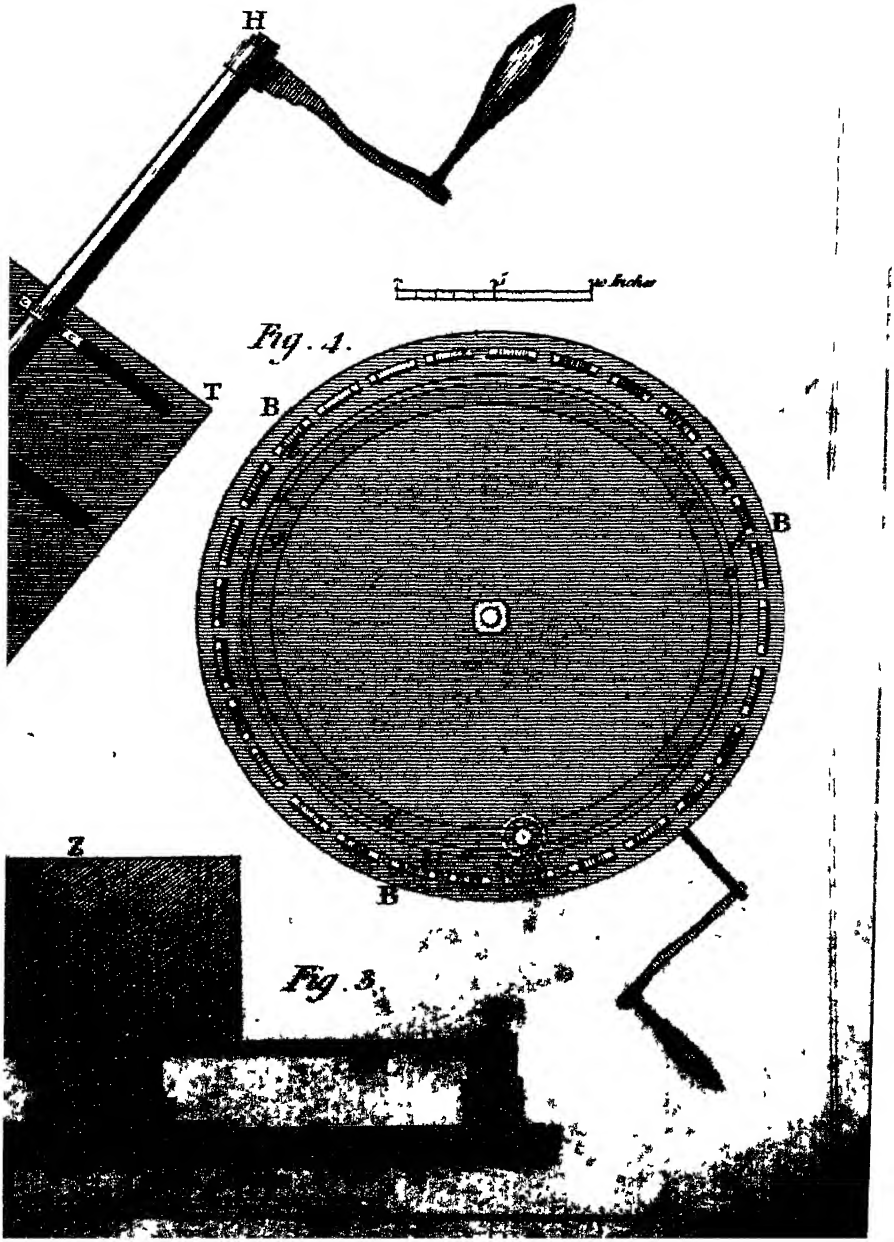
|| PLIN. lib. XXXIII. cap. ix. and lib. XXXIV. cap. xvii.

¶ Ibid.

** Ibid.

†† VITRUVIUS, lib. VII. cap. viii.

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I. Observations on the chemical Nature of the Humours of the Eye. By Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read November 5, 1802.

THE functions of the eye, so far as they are physical, have been found subject to the common laws of optics. It cannot be expected that chemistry should clear up such obscure points of physiology, as all the operations of vision appear to be; but, some acquaintance with the intimate nature of the substances which produce the effects, cannot fail to be a useful appendage to a knowledge of the mechanical structure of the organ.

The chemical history of the humours of the eye, is not of much extent. The aqueous humour had been examined by BERTRANDI; who said, that its specific gravity was 975, and therefore less than that of distilled water. FOURCROY, in his *Système des Connoissances chimiques*, tells us, that it has a saltish taste; that it evaporates without leaving a residuum; but that it contains some animal matter, with some alkaline phosphate and muriate. These contradictions only prove, that we have no accurate knowledge upon the subject.

The vitreous humour is not better known. WINTRINGHAM has given its specific gravity (taking water at 10000) as equal to 10024; but I am not acquainted with any experiments to investigate its chemical nature.

We are told by CHROVET, that the crystalline lens a by destructive distillation, fetid oil, carbonate of ammonia, and

water, leaving some carbon in the retort. But, destructive distillation, although it has given us much knowledge as to animal matter in general, is too vague a method for investigating particular animal substances.

I shall now proceed to mention the experiments I have made upon all the humours. I shall first relate those which were made upon the eyes of sheep, (they being the most easily procured,) and shall afterwards speak of those of the human body, and other eyes. I think it right to observe, that all these eyes were as fresh as they could be obtained.

SHEEPS' EYES.

Aqueous Humour.

The aqueous humour is a clear transparent liquid, of the specific gravity of 10090,* at 60° of FAHRENHEIT. When fresh, it has very little smell, or taste.

It causes very little change in the vegetable reactive colours; and this little would not, I believe, be produced immediately after death. I imagine it to be owing to a generation of ammonia, some traces of which I discovered.

When exposed to the air, at a moderate temperature, it evaporates slowly, and becomes slightly putrid.

When made to boil, a coagulum is formed, but so small as hardly to be perceptible. Evaporated to dryness, a residuum remains, weighing not more than 8 per cent. of the original liquor.

Tannin causes a precipitate in the fresh aqueous humour,

* All these specific gravities are mean proportionals of several experiments. The eyes of the same species of animal, do not differ much in the specific gravity of their

both before and after it has been boiled, and consequently shows the presence of gelatine.

Nitrate of silver causes a precipitate, which is muriate of silver. No metallic salts, except those of silver, alter the aqueous humour.

From these and other experiments it appears, that the aqueous humour is composed of water, albumen, gelatine, and a muriate, the basis of which I found to be soda.

I have omitted speaking of the action of the acids, of the alkalis, of alcohol, and of other reagents, upon this humour. It is such as may be expected in a solution of albumen, of gelatine, and of muriate of soda.

Crystalline Humour.

To follow the order of their situation, the next of the humours is the crystalline.

This differs very materially from the others.

Its specific gravity is 1.1000.

When fresh, it is neither acid nor alkaline. It putrifies very rapidly. It is nearly all soluble in cold water, but is partly coagulated by heat. Tannin gives a very abundant precipitate; but I could not perceive any traces of muriatic acid, when I had obtained the crystalline quite free from the other humours. It is composed, therefore, of a smaller proportion of water than the others, but of a much larger proportion of albumen and gelatine.

Vitreous Humour.

I pressed the vitreous humour through a rag, in order to free it from its capsules; and, in that state, by all the experiments I could make upon it, I could not perceive any difference between it and the aqueous humour, either in its specific gravity,

(which I found to be 10090, like that of the other,) or in its chemical nature.

M. Fourcroy mentions a phosphate, as contained in these humours; but I could not perceive any precipitation by muriate or nitrate of lime; nor did the alkalis denote the presence of any earth, notwithstanding M. Fourcroy's assertion of that fact.

HUMAN EYE.

I could not procure a sufficient quantity of these, fresh enough to multiply my experiments upon them. However, by the assistance of Mr. Carpue, Surgeon to his Majesty's Forces, I fully convinced myself, that the humours of the human eye, chemically considered, did not contain any thing different from the respective humours of the eyes I had examined. The aqueous and vitreous humours contained water, albumen, gelatine, and muriate of soda; and the crystalline humour contained only water, albumen, and gelatine. The specific gravity of the aqueous and vitreous humours, I found to be 10053; while that of the crystalline was 10790.

EYES OF OXEN.

I found the eyes of oxen to contain the same substances as the respective humours of other eyes. The specific gravity of the aqueous and vitreous humours is 10088; and that of the [illegible].

What is particularly worthy of notice is, that the difference which appears to exist between the specific gravity of the aqueous and vitreous humour and that of the crystalline, is much greater in the human eye than in that of sheep, and less in the eye of oxen. Hence it would appear, that the difference between the density of the aqueous and vitreous humour and that of the crys-

talline, is in the inverse ratio of the diameter of the eye, taken from the cornea to the optic nerve. Should further experiments show this to be a universal law in nature, it will not be possible to deny that it is in some degree designed for the purpose of promoting distinct vision.

In taking the specific gravity of the aqueous and vitreous humours, no particular precaution is necessary, except that they ought to be as fresh as possible. But the crystalline humour is not of an uniform density throughout; it is therefore essential, that attention be given to preserve that humour entire for this operation. I found the weight of a very fresh crystalline of an ox to be 30 grains; and its specific gravity was, as I before stated, 10765. I then pared away all the external part, in every direction, till there remained but 6 grains of the centre; and the specific gravity of these 6 grains, I found to be 11940. From this it would seem, that the density increases gradually, from the circumference to the centre.

It is not surprising that the crystalline humour should be subject to disorders, it being wholly composed of animal matter of the most perishable kind. FOURCROY says, that it is sometimes found osseous in advanced age. Albumen is coagulated by many methods; and, if we suppose that the same changes can take place in the living eye as in the dead animal matter of the chemists, it will be easy to account for the formation of the cataract; a disorder which cannot be cured but by the removal of the opaque lens. If a sufficient number of observations were made respecting the frequency of the cataract in gouty habits, some important conclusions might be drawn, as to the influence of phosphoric acid, in causing the disorder, by the common effect of acids, in coagulating albumen.

VI. *An Account of some Stones said to have fallen on the Earth in France; and of a Lump of native Iron, said to have fallen in India. By the Right Hon.* Charles Greville, *F. R. S.*

Read January 27, 1803.

The experiments and observations made by Edward Howard, Esq. on certain stony and metalline substances said to have fallen on the earth, and the accurate description which the Count de Bournon has given of those substances, have, in my opinion, fully established the following fact, namely, that a number of stones asserted to have fallen under similar circumstances, have precisely the same characters.

The stones from Benares, that from Yorkshire, that from Sienna, and that from Bohemia, were the whole which had then been seen in England. They all contained pyrites of a peculiar character: they all had a coating of black oxide of iron: they all contained an alloy of iron and nickel; and the earths which served to them as a sort of connecting medium, corresponded in their nature, and nearly in their proportions.

[illegible] of Mr. Howard's and Count de [illegible] I have received from France three [illegible] St. Amand very obligingly [illegible] had broken from a stone of [illegible] preserved in the Museum of Bour-[illegible] Roquefort, in the Landes, on the [illegible] the explosion of a meteor: it broke

through the roof of a cottage, and killed a herdsman and some cattle. M. ST.-AMAND also gave me part of a stone he had preserved in his collection ever since the year 1790, when a shower of stones, weighing from ½ an ounce to 15 and 25 pounds each, fell in the parishes of Grange and Creon, and also in the parish of Julice, in Armagnac; which fact was, at the time, verified by DUBY, Mayor of Armile, and published by BERTHOLON, in the *Journal des Sciences utiles de Montpellier*, in the year 1790.

The third specimen, I owe to the Marquis de DREE; it is a fragment, broken from a stone of 22 pounds weight, which fell near the village Salles, not far from Villefranche in Burgundy, on the 12th of March, 1798; this was also accompanied by a meteor.

I content myself with the mere recital of the facts, in confirmation of the observations presented to the Society, as these three additional specimens have precisely the same characters, texture, and appearance, as the others in my collection; and are scarcely, by the eye, to be distinguished from them.

I should not, perhaps, have troubled the Society with this account, as my friend the Marquis de DREE, whose knowledge in mineralogy peculiarly qualifies him to investigate these subjects, has given me hopes of seeing his observations on them published; but a new evidence has lately fallen into my hands, and is the only one I have met with that ascertains the origin of native iron, which, from analysis, had been suspected to have a common origin with the stones fallen on the earth. Conversing with Colonel KIRKPATRICK, whose researches have embraced both the literature and politics of India, and whose tale[illegible]ous

parts of India, I inquired whether he had ever heard of any instances similar to the explosion of the meteor at Benares in 1798. He told me, he could not recollect having heard or read of any other instance, excepting one in the Memoirs written by the Emperor Jehangire, and of that he did not recollect the particulars. A few days after, having found the passage in the original Persian, he was so obliging as to translate it. I consider it as an authentic fact; for the Emperor Jehangire was not a prince on whom his courtiers would idly venture to impose; and there can be little probability that an Aumil of a district should invent such a story, or be able to produce a substance apparently like iron, but which, on trial, differed from manufactured iron. Colonel Kirkpatrick's translation, I have obtained his leave to communicate, with his attestation, to the Royal Society.

Extract from the Memoirs of the Emperor Jehangire, *written (in Persian) by himself, and translated by Colonel* Kirkpatrick.

A. H. 1030, *or 16th year of the reign.*—The following is among the extraordinary occurrences of this period.

Early on the 30th of Furverdeen, of the present year,* and in the Eastern quarter, [of the heavens] there arose in one of the villages of the Purgunnah of Jalindher,† such a great and tremendous noise as had nearly, by its dreadful nature, deprived the inhabitants of the place of their senses. During this noise, a luminous body [was observed] to fall from above on the

* The first of Furverdeen of this year, (A. H. 1030,) corresponded with Saturday, the [illegible] of [illegible]; consequently, the 30th of Furverdeen fell on the 26th of Jumnad ul Owwul, or A. H. [illegible].

† A purgunnah is a territorial division, of arbitrary extent. The purgunnah of Jalindher is situated in the Punjaub, and about 100 miles S. E. of Lahore.

earth, suggesting to the beholders the idea that the firmament was raining fire. In a short time, the noise having subsided, and the inhabitants having recovered from their alarm, a courier was dispatched [by them] to MAHOMMED SYEED, the Aumil* of the aforesaid Purgunnah, to advertise him of this event. The Aumil, instantly mounting, [his horse,] proceeded to the spot, [where the luminous body had fallen]. Here he perceived the earth, to the extent of ten or twelve guz,† in length and breadth, to be burnt to such a degree, that not the least trace of verdure, or a blade of grass remained; nor had the heat [which had been communicated to it] yet subsided entirely.

MAHOMMED SYEED hereupon directed the aforesaid space of ground to be dug up; when, the deeper it was dug the greater was the heat of it found to be. At length, a lump of iron made its appearance, the heat of which was so violent, that one might have supposed it to have been taken from a furnace. After some time it became cold; when the Aumil conveyed it to his own habitation, from whence he afterwards dispatched it, in a sealed bag, to court.

Here I had [this substance] weighed in my presence. Its weight was one hundred and sixty tolahs.‡ I committed it to a skilful artisan, with orders to make of it a sabre, a knife, and a dagger. The workman [soon] reported, that the substance was not *malleable, but shivered into pieces under the hammer.* §

Upon this, I ordered it to be mixed with other iron. Con-

* Aumil is a manager or fiscal superintendant of a district.

† A guz is rather less than a yard.

‡ A tolah is about 180 grains, Troy weight.

§ Literally, "it did not stand beneath the hammer, but fell to pi

formably to my orders, three parts of the *iron of lightning** were mixed with one part of common iron; and from the mixture were made two sabres, one knife, and one dagger.

By the addition of the common iron, the [new] substance acquired a [fine] temper; the blade [fabricated from it] proving as elastic as the most genuine blades of Ulmanny,† and of the South, and bending, like them, without leaving any mark of the bend. I had them tried in my presence, and found them cut excellently; as well [indeed] as the best genuine sabres. One of these sabres I named *Katai*, or *the cutter*; and the other *Burk-serisht*, or the *lightning-natured.*

A poet‡ composed and presented to me, on this occasion, the following tetrastich.

"This earth has attained order and regularity through the
"Emperor JEHANGIRE:
"In his time fell *raw* iron from lightning:
"That iron was, by his world-subduing authority,
"Converted into a dagger, a knife, and two sabres."

The chronogram of this occurrence is contained in the words شعلهٔ برق بادشاهي, which signify "the flame of the imperial lightning;" and give the year (of the Hegera) 1030.

N. B. The foregoing translation (which is nearly literal) has been made from a manuscript that has been several years in my possession; and which, although without date, bears marks of having been written at a remote period.

WM. KIRKPATRICK.

* This expression is equivalent to our term *thunder-bolt.*

† The name of the place here designed is doubtful.

‡ The poet is named in the original; but the name is not perfectly legible.

VII. *Observations on the Structure of the Tongue; illustrated by Cases in which a Portion of that Organ has been removed by Ligature. By* Everard Home, *Esq. F. R. S.*

Read February 3, 1803.

Physiological inquiries have ever been considered as deserving the attention of this learned Society; and, whenever medical practitioners, in the treatment of diseases, have met with any circumstance which threw light upon the natural structure or actions of any of the organs of the human body, or those of other animals, their communications have met with a favourable reception.

The following observations derive their real importance from offering a safe and effectual means of removing a portion of the tongue, when that organ has taken on a diseased action, the cure of which is not within the reach of medicine; and, as the tongue, like many other glandular structures, is liable to be affected by cancer, it becomes of no small importance that the fact should be generally known. In a physiological view, they tend to show, that the internal structure of the tongue is not of that delicate and sensible nature which, from its being the organ of taste, we should be led to imagine.

The tongue is made up of fasciculi of muscular fibres, with an intermediate substance met with in no other part of the body, and a vast number of small glands; it has large nerves passing through it; and the tip possesses great sensibility, fitting it for the purpose of taste.

Whether the sense of taste is confined entirely to the point of the tongue, and the other parts are made up of muscles fitted for giving it motion; or whether the whole tongue is to be considered as the organ, and the soft matter which pervades its substance, and fills the interstices between the fasciculi of muscular fibres, is to be considered as connected with sensation, has not, I believe, been ascertained.

The tongue, throughout its substance, has always been considered by physiologists as a very delicate organ; and it was believed, that any injury committed upon it would not only produce great local irritation, but also affect, in a violent degree, the general system of the body. This was my own opinion, till I met with the following case, the circumstances of which induced me to see this organ in a different point of view.

A gentleman, by an accident which it is unnecessary to describe, had his tongue bitten with great violence The immediate effect of the injury was great local pain; but it was not attended with much swelling of the tongue itself, nor any other symptom, except that the point of the tongue entirely lost its sensibility, which deprived it of the power of taste: whatever substance the patient eat was equally insipid. This alarmed him very much, and induced him to state to me the circumstances of his case, and request my opinion. I examined the tongue, a fortnight after the accident. It had the natural appearance, but the tip was completely insensible, and was like a piece of board in his mouth, rendering the act of eating a very unpleasant operation. I saw him three months afterwards, and it was still in nearly the same state.

From this case it appears, that the tongue itself is not particularly irritable; but the nerves passing through its substance

to supply the tip, which forms the organ of taste, are very readily deprived of their natural action; this probably arises from their being softer in texture than nerves in general, and, in that respect, resembling those belonging to the other organs of sense.

There was another circumstance in this case which very particularly struck my attention, *viz.* that a bruise upon the nerves of the tongue, sufficient to deprive them of the power of communicating sensation, was productive of no inflammation or irritation in the nervous trunk, so as to induce spasms, which too commonly occur from injuries to the nerves belonging to voluntary muscles. I am therefore led to believe, that the nerves supplying an organ of sense, are not so liable to such effects as those which belong to the other parts of the body.

The small degree of mischief which was produced, and the readiness with which the nerves had their communication completely cut off, were to me new facts, and encouraged me, in the following case of fungous excrescence from the tongue, which bled so profusely as at times to endanger the patient's life, and never allowed him to arrive at a state of tolerable health, to attempt removing the part by ligature.

John Weymouth, eight years of age, was admitted into St. George's hospital, on the 24th of December, 1800, on account of a fungous excrescence on the right side of the anterior part of the tongue, which extended nearly from the outer edge to the middle line at the tip. It appeared, from the account of his relations, that the origin of this fungus existed at his birth, and had been increasing ever since. He had been a year and a half under the care of the late Mr. Cruikshank, who had removed

the excrescence by ligature round its base; but, when the ligature dropped off, a violent hæmorrhage took place, and the excrescence gradually returned. Attempts were made to destroy it by caustic; but hæmorrhage always followed the separation of the sloughs; so that, after ten trials, this mode was found ineffectual. It was also removed by the knife, ten different times, but always returned.

From this history I was led to believe, that the only mode of removing the disease was taking out the portion of the tongue upon which it grew. This was a case in which I felt myself warranted in making an attempt out of the common line of practice, to give the patient a chance of recovery; and, from the preceding case, having found that pressure on one part of the tongue produced no bad consequences on the other parts, I was led to remove the excrescence in the following manner.

On the 28th of December, I made the boy hold out his tongue, and passed a crooked needle, armed with a double ligature, directly through its substance, immediately beyond the excrescence. The needle was brought out below, leaving the ligatures; one of these was tied very tight before the excrescence, the other equally so beyond it, so that a segment of the tongue was confined between these two ligatures, in which the circulation was completely stopped. The tongue was thin in its substance; and the boy complained of little pain during the operation. Thirty drops of laudanum were given to him immediately after it, and he was put to bed. He fell asleep, continued to dose the greater part of the day, and was so easy the next day as to require no particular attention. On the fifth day from the operation, the portion of tongue came away with the ligatures, leaving a sloughy surface which was thrown off on the 11th day, and was

succeeded by a similar slough; this separated on the 15th day. The excavation after this gradually filled up; and, on the 20th day, it was completely cicatrized, leaving only a small fissure on that side of the tongue.

Encouraged by the result of this case, I was led to perform a similar operation upon a person at a more advanced period of life.

MARGARET DALTON, 40 years of age, was admitted into St. George's hospital, on the 25th of December, 1801, on account of a tumour, the size of a pea, situated on the right side of the tongue, near its edge. The history of the case was as follows. A small pimple appeared, and gradually increased, without pain; the only inconvenience was, that it affected her speech, and, when bruised by the teeth, bled freely.

The operation was performed on the 11th of January, 1802, in exactly the same manner as has been already described. It produced a considerable degree of salivation, which was extremely troublesome, (much more so than the pain the ligatures produced,) and continued till the slough came away. The ligature nearest the root of the tongue separated on the 6th day; the other on the 7th; and, in three days after the separation of the second ligature, the wound was completely skinned over.

A third case of this kind came under my observation, in which there was a small tumour in the substance of the tongue, about the size of a pea, which gave me the idea of its being of that kind which might terminate in cancer. The patient was a gentleman of about 41 years of age. Upon examining the tumour, I told him of my alarm respecting its nature; and at the same time added, that I was very ready to remove it, should it be

the opinion of other practitioners that such a step was adviseable; and my experience in two former cases led me to believe it might be done with safety. I therefore advised him to consult other medical practitioners of reputation, and acquaint me with their opinion. Mr. Cline was consulted, and his opinion coincided with mine; which made the patient decide upon having the tumour removed.

The operation was performed on the 28th of December, 1802. The needle pierced the tongue an inch beyond the tip, a little to the right of the middle line of the tongue; and the space between the two ligatures, when they were tied at the circumference of the tongue, was fully an inch. The tongue was thick; and the mass included by the ligatures was such as to make it difficult to compress it. The operation gave considerable pain, of a numbing kind. Immediately after the operation, the part included became dark coloured, particularly towards the middle line of the tongue. A salivation took place. The next day, the pain and salivation were great, and the patient could not swallow; but, on the day following, he could take broth, negus, and other fluids.

On the 6th day from the operation, the slough became loose; and the least motion of the tongue gave great pain. Upon examining the slough, there was a small spot which looked red, and was surrounded by a dark surface; this was towards the right side. Upon further examination it appeared, that the ligature to the right had not completely deadened the part at the centre, in which the artery had its course. This accounted for the red spot, as well as for the pain the patient suffered; and led me, on the seventh day, to disengage the ligature on the left,

(which was almost completely separated,) by means of a pair of scissors, and pass another ligature through the groove to the opposite side, and tie it over the part not completely deadened. This gave great pain for a few hours, which was relieved by the use of tincture of opium. On the 8th day, the patient had less pain than on any preceding day, and less salivation; and, on the 9th, the whole slough came away. On the 13th, the tongue had so much recovered itself, that there did not appear any loss of substance whatever, only a fissure of half an inch in depth, in the anterior part of it; and, as that now seemed to be exactly in the centre, there was not the smallest deformity.

The preceding cases, in the view which it is intended to take in the present Paper, are to be considered as so many experiments, by which the structure of the tongue is in some respects ascertained: they enable us to draw the following conclusions.

The internal structure of the tongue is less irritable than almost any other organized part of the body; therefore, the peculiar substance which is interposed between the fasciculi of its muscular fibres, is not in any respect connected with the nerves which pass through its substance to the organ of taste, but is merely a soft medium, to admit of great facility of action in its different parts.

The nerves of the tongue appear to be more readily compressed, and deprived of their power of communicating sensation, than nerves in general; and any injury done to them is not productive of diseased action in the trunk of the injured nerve.

If we compare the effects of compression upon a portion of the tongue, with those of a similar compression upon the hæmorrhoidal veins when they form piles, or those of the testicle in cases of varicose veins of the spermatic chord, which not only produce very violent local inflammation, but also a considerable degree of symptomatic fever, it is impossible not to be surprised that the results should be so very different; since we are led to believe, upon a general principle, that parts are sensible in proportion to their vascularity, and that all the organs of sense, when inflamed, are more exquisitely so than any other parts of the body.

The tongue appears to have a power of throwing off its sloughs in a shorter time than any other part. Eight or nine days is the ordinary time of a slough separating from the common parts: in the boy's tongue, it was only five.

Having stated the information we derive from these cases, respecting the structure, sensibility, and irritability of the tongue, it now remains to mention the advantage to be derived from them in a professional view; and, although this is not directly in the line of the pursuits of this learned Society, yet, so strongly is it connected with humanity, that it cannot be said to be foreign to them, or undeserving their attention.

The information derived from these cases, enables us to attempt with safety, the removal of any part of the tongue which may have taken on a disposition to become cancerous. As this disease in the tongue always begins in a very small portion of that organ, it is, in the early stage, more within the reach of removal than when in any other part of the body; and, as the glands of the tongue are independent of each other, the cancerous

disposition by which one of them is attacked, does not so readily communicate itself to the others; and the part may be removed, with a greater degree of security against a future recurrence of the disease, than in other cases where this malady attacks a portion of a large gland, the whole of which may be under the influence of the poison, long before there is any appearance of its being diseased.

VIII. *Observations of the Transit of Mercury over the Disk of the Sun; to which is added, an Investigation of the Causes which often prevent the proper Action of Mirrors. By* William Herschel, *LL. D. F. R. S.*

Read February 10, 1803.

THE following observations were made with a view to attend particularly to every phenomenon that might occur during the passage of the planet Mercury over the sun's body. My solar apparatus, on account of the numerous observations I have lately been in the habit of making, was in great order for viewing the sun in the highest perfection; and, very fortunately, the weather proved to be as favourable as I could possibly have wished it.

The time at which the observations were made, not being an object of my investigation, is only to be considered as denoting the order of their succession.

November 9, 1802. About 40′ after seven o'clock in the morning, I directed a telescope, with a glass mirror of 7 feet focal length, and 6,3 inches in diameter, to the sun; and perceived the planet Mercury. It was easily to be distinguished from the openings in the luminous clouds, generally called spots, of which there were more than forty in number. Its perfect roundness would have been sufficient to point it out, had I not already known where to look for it.

$10^h\ 0'$. When the sun was come to a sufficient altitude to show objects on its surface with distinctness, I directed my

attention to the contour of the mercurial disk, and found its termination perfectly sharp.

With a 10-feet reflector, and magnifying power of 130, I saw the corrugations of the luminous solar surface, up to the very edge of the whole periphery of the disk of Mercury.

10^h 27′. When the planet was sufficiently advanced towards the largest opening of the northern zone, I compared the intensity of the blackness of the two objects: and found the disk of Mercury considerably darker, and of a more uniform black tint, than the area of the large opening.

10^h 32′. The preceding limb of Mercury cuts the luminous solar clouds with the most perfect sharpness; whereas, in the great opening, the descending parapet, down the preceding side, was plainly visible.

It should be remarked, that the instrument here applied to the sun, with the moderate power of 130, is the same 10-feet reflector which, in fine nights, when directed to very minute double stars, will show them distinctly with a magnifier of 1000.

Having often attempted to use high magnifiers in viewing the sun, I wished to make another trial; though pretty well assured I should not succeed, for reasons which will appear hereafter.

With two small double convex lenses, both made of dark green glass, and one of them having the side which is nearest the eye thinly smoked, in order to take off some light, I viewed the sun. Their magnifying power was about 300; and I saw Mercury very well defined; but that complete distinctness, which enables us to judge with confidence of the condition of the object in view, was wanting.

With a single eye-glass, smoked on the side towards the eye, and magnifying 460 times, I also saw Mercury pretty well

defined; but here the sun appeared ruddy, and no very minute objects could be perceived.

$11^h\ 28'$. The planet having advanced towards the preceding limb of the sun, it was now time to attend to the appearances of the interior and exterior contacts.

$11^h\ 32'$. 10-feet reflector. The whole disk of Mercury is as sharply defined as possible; there is not the least appearance of any atmospheric ring, or different tinge of light, visible about the planet.

$11^h\ 37'$. Appearances remain exactly as before.

$11^h\ 42'$. The sharp termination of the whole mercurial disk, appears to be even more striking than before. This may be owing to its contrast with the bright limb of the sun, which, having many luminous ridges in the northern zone, is remarkably brilliant about the place of the planet.

$11^h\ 44'$. I was a few moments longer writing down the above than I should have been, to see the interior contact so completely as I could have wished; however, the thread of light on the sun's limb was but just breaking, or broken; but no kind of distortion, either of the limb or of the disk of Mercury, took place.

The appearance of the planet, during the whole time of its emerging from the sun, remained well defined, to the very last.

The following limb of Mercury remained sharp, till it reached the very edge of the sun's disk; and vanished without occasioning the smallest distortion of the sun's limb, in going off, or suffering the least alteration in its own figure.

As soon as the planet had quitted the sun, the usual appearance of its limb was so instantly and perfectly restored, that not the least trace remained whereby the place of its disappearance

could have been distinguished from any other adjacent part of the solar disk.

It will not be amiss to add, that very often, during the transit, I examined the appearance of Mercury with a view to its figure, but could not perceive the least deviation from a spherical form; so that, unless its polar axis should have happened to be situated, at the time of observation, in a line drawn from the eye to the sun, the planet cannot be materially flattened at its poles.

OBSERVATIONS AND EXPERIMENTS RELATING TO THE CAUSES WHICH OFTEN AFFECT MIRRORS, SO AS TO PREVENT THEIR SHOWING OBJECTS DISTINCTLY.

It is well known to astronomers, that telescopes will act very differently at different times. The cause of the many disappointments they may have met with in their observations, is however not so well understood.

Sometimes we have seen the failure ascribed to certain tremors, as belonging to specula; and remedies have been pointed out for preventing them. Not unfrequently again, the telescope itself has been condemned; or, if its goodness could not admit of a doubt, the weather in general has been declared bad, though possibly it might be as proper for distinct vision as any we can expect in this changeable climate.

The experience acquired by many years of observation, will however, I believe, enable me now to assign the principal cause of the disappointments to which we are so often exposed Unwilling to hazard any opinion that is not properly supported by facts, I shall have recourse to a collection of occasional observations. They have been made with specula of undoubted

goodness, so that every cause which impeded their proper action must be looked upon as extrinsic. I shall arrange these observations under different heads, that, when they have been related, there may remain no difficulty to draw a few general conclusions from them, which will be found to throw a considerable light upon our subject.

Moisture in the Air.

(1.) October 5, 1781. I see double stars, with 460, completely well. The air is very damp.

(2.) Nov. 23, 1781. $15^h\ 30'$. The morning is uncommonly favourable, and I see the treble star ζ Cancri, with 460, in high perfection. The air is very moist, and intermixed with passing clouds.

(3.) Sept. 7, 1782. I viewed the double star preceding 12 Camelopardalis,* with 932. In this, and several other fine nights which I have lately had, the condensing moisture on the tube of my telescope has been running down in streams; which proves that damp air is no enemy to good vision.

(4.) Dec. 28, 1782. $17^h\ 30'$. The water condensing on my tube keeps running down; yet I have seen very well all night. I was obliged to wipe the object-glass of my finder almost continually. The specula, however, are not in the least affected with the damp. The ground was so wet that, in the morning, several people believed there had been much rain in the night, and were surprised when I assured them there had not been a drop.

(5.) Feb. 19, 1783. I have seen perfectly well till now† that

* See Phil. Trans. Vol. LXXV. Part I. page 68; II. 53.

† The time is not marked in the journal; but, from the number of the observations that had been made during the night, it must have been towards morning.

a frost is coming on; though Datchet Common, which is just before my garden, is all under water; and the grass on which I stand with my telescope is as wet as possible.

(6.) Feb. 26, 1783. All the ground is covered with snow: yet I see remarkably well.

(7.) March 8, 1783. The common before my garden is all under water; my telescope is running with condensed vapour; not a breath of air stirring. I never saw better.

(8.) August 25, 1783. My telescope ran with water all the night. The small speculum, which sometimes gathers moisture, was never affected in the 7-feet tube, but was a little so in the 20-feet. The large eye-glasses and object-glasses of the finders, required wiping very often. I saw all night remarkably well.

Fogs.

(9) Oct. 30, 1779. It grows very foggy, and the moon is surrounded with strong nebulosity; nevertheless, the stars are very distinct, and the telescope will bear a considerable power.

(10.) August 20, 1781. It is so foggy that I cannot see an object at the distance of 40 feet; yet the stars are very distinct in the telescope. By an increase of the fog, α Piscium can no longer be seen by the eye; yet, in the telescope, it being double, I see both the stars with perfect distinctness.

(11.) Sept. 6, 1781. A fog is come on; yet I see very well.

(12.) Sept. 9, 1781. There is so strong a fog, that hardly a star less than 30° high is to be seen; and yet, in the telescope, at great elevations, I see extremely well.

(13.) March 9, 1783. It is very foggy; yet in the telescope I see the stars without aberration, and they are very bright. α Serpentarii is without a single ray.

(14.) April 6, 1783. A very thick fog settles upon all my glasses; but the specula, even the 20-feet, which has so large a surface, remain untouched. I see perfectly well.

Frost.

(15.) Nov. 15, 1780; 5 o'clock in the morning. An excellent speculum, No. 2, will not act properly; the frosty morning probably occasions an alteration in its figure. Another speculum, No. 1, acts but indifferently, though I have known it to shew very well formerly in a very hard frost: for instance, November 23, 1779, I saw with the same mirror, and a power of 460, the vacancy between the two stars of the double star Castor, without the least aberration.

(16.) Oct. 22, 1781. Frost seems to be no hindrance to perfect vision. The tube of my 7-feet telescope is covered with ice; yet I see very well.

(17.) Nov. 19, 1781. It freezes very hard, and the stars, even those which are 50° high, are very tremulous. I suspect their apparent diameters to be diminished; and, if I recollect right, this is not the first time that such a suspicion has occurred to me.

(18.) Jan. 10, 1782. My telescope would not act well, even at an altitude of 70 or 80 degrees. There is a strong frost.

(19.) Jan. 31, 1782. I cannot see with a power of 460, the stars seem to dance so unaccountably, and yet the air is perfectly calm: even at 60 or 70 degrees of altitude, vision is impaired.

(20.) Feb. 9, 1782. That frost is no hindrance to seeing well is evident; for, not only my breath freezes upon the side

of the tube, but more than once have I found my feet fastened to the ground, when I have looked long at the same star.

(21.) Oct. 4, 1782. It froze very severely this night. At first, when the frost came on, I saw very badly, every object being tremulous; but, after some time, and at proper altitudes, I saw as well as ever. Between 5 and 6 o'clock in the morning, objects began to be tremulous again; occasioned, I suppose, by the coming on of a thaw.

(22.) Jan. 1, 1783. I made a number of delicate observations this night, notwithstanding, at 4 o'clock in the morning, my ink was frozen in the room; and, about 5 o'clock, a 20-feet speculum, in the tube, went off with a crack, and broke into two pieces. On looking at FAHRENHEIT's thermometer, I found it to stand at 11°.

(23.) May 6, 1783. It freezes, and in the telescope the stars seem to dance extremely.

Hoar-frost.

(24.) Nov. 6, 1782. There is a thick hoar-frost; yet I see extremely well. It seems to enlarge the diameters of the stars; but, as I see the minutest double stars well, the apparent enlargement of the diameters must be a deception.

(25.) Dec. 22, 1782. There is a strong hoar-frost gathering upon the tubes of my telescopes; but I see very well.

Dry Air.

(26.) Dec. 21, 1782. The tube of my telescope is dry, and I do not see well.

(27.) April 30, 1783. The stars are extremely tremulous

and confused; the outside of the tube of my telescope is quite dry.

Northern Lights.

(28.) Sept. 25, 1781. There are very strong northern lights; their flashing does not seem to interfere with telescopic vision; but all objects appear tremulous, and indifferently defined.

(29.) Aug. 30, 1782. There are very bright northern lights, in broad arches, with white streaks; yet I see perfectly well.

(30.) March 26, 1783. An Aurora Borealis is so bright, that η Herculis, which it covers, can hardly be seen; yet, in the telescope, and with a power of 460, I find no difference. I compared that star with γ Coronæ, which was in a bright part of the heavens, and in the telescope they appeared nearly alike. I suspected η Herculis to be somewhat more tinged with red than it should be; and examined it afterwards, when clear of the Aurora: it was indeed less red; but, as it had gained more altitude, the experiment was not decisive.

Windy Weather.

(31.) Jan. 8, 1783. It is very windy. The diameters of the stars are strangely increased, even those at 60 and 70° of altitude. Every star seems to be a little planet.

(32.) Jan. 9, 1783. Wind increases the apparent diameters of the stars.

(33.) Sept. 20, 1783. The night has been very windy; and I do not remember ever to have seen so ill, with such a beautiful appearance of brilliant star-light.

Fine in Appearance.

(34.) May 28, 1781. The evening, though fine in appearance,

is not favourable. No instrument I have will act properly. The wind is in the east.

(35.) August 30, 1781. The stars appear fine to the naked eye, so that I can see ε Lyræ very distinctly to be two stars; yet my telescope will show nothing well. There are flying clouds, which, by their rapid motion, indicate a disturbance in the upper regions of the air; though, excepting now and then a few gusts of wind, it is in general very calm. At a distance there are continual flashes of lightning, but I can hardly hear any thunder.

(36.) Sept. 14, 1781. I see very small stars with the naked eye; but the telescope will not act so well as it should.

(37.) Sept. 24, 1781. The evening is apparently fine; but, with the telescope, I can see neither η Coronæ nor μ Bootis double; nor indeed can I see any other stars well.

Over a Building.

(38.) August 24, 1780. I viewed ε Bootis with 449, 737, and 910, but saw it very indifferently. The star was over a house.

(39.) Oct. 26, 1780. ε Bootis being near the roof of a house, I saw it not so distinctly as I could wish.

The Telescope lately brought out.

(40.) Oct. 10, 1780. 6h 30′. Having but just brought out my telescope, it will not act well.

6h 45′. The tube and specula are now in order, and perform very well.

(41.) Jan. 11, 1782. To all appearance, the [illegible] was very fine, but still the telescope, when first brought out, would not act well. After half an hour's exposure, it [illegible].

Confined Place.

(42.) July 19, 1781. 13^h $15'$. My telescope would not act well; and, supposing the exhalations from the grass in my garden to affect vision, I carried the telescope into the street, (the observation was made at Bath,) and found it to perform to admiration.

(43.) July 19, 1781. My telescope acted very well; but a slight field-breeze springing up, and brushing through the street where my instrument was placed, it would no longer bear a magnifying power of 460.

Haziness and Clouds.

(44.) Sept. 22, 1783. The weather is now so hazy, that the double star δ Cygni is but barely visible to the naked eye. This has taken off the rays of the large star, so that I now see the small one extremely well, which at other times it is so difficult to perceive, even with a magnifying power of 932.

(45.) August 13, 1781. A cloud coming on very gradually upon fixed stars, has this remarkable effect, that their apparent diameters diminish gradually to nothing.

(46.) July 7, 1780. The air was very hazy, but extremely calm. I had Arcturus in the field of view of the telescope, and, the haziness increasing, it had a very beautiful effect on the apparent diameter of this star. For, supposing the first of the points, Plate III. Fig. 1, to represent its magnitude when brightest, I saw it gradually decrease, and assume, with equal distinctness, the form of all the succeeding points, from No. 1 to No. 10, in the order of the numbers placed over them. The last magnitude I saw it under, could certainly not exceed two-

tenths of a second; but was perhaps less than one. This leads to the discovery of one of the causes of the apparent magnitude of the fixt stars.

Focal Length.

(47.) Nov. 14, 1801. The focal length of my 10-feet mirror increases by the heat of the sun. I have often observed this before; the difference, by several trials, amounts to 8 hundredths of an inch.

(48.) Dec. 13, 1801. The focal length of my 10-feet mirror, while I was looking at the sun, became shorter, contrary to what it used to do; but, there being a strong frost, I guess that the object metal grows colder, notwithstanding its exposure to the sun's rays.

(49.) Nov. 9, 1802. 10^h $50'$. The focus of my 7-feet glass mirror became 18 hundredths of an inch shorter, on being exposed for about a minute to the sun. The figure of the speculum was also distorted; the foci of the inside and outside rays differing considerably, though its curvature, by observations on the stars, has been ascertained to be strictly parabolical.

12^h $0'$. The same mirror, exposed one minute to the action of the sun, became 21 hundredths shorter in focal length.

The focus of a 10-feet metalline mirror, when exposed one minute to the sun's rays, became 15 hundredths of an inch longer than it was before.

(50.) January 9, 1803. When I looked with the glass 7-feet mirror, several times, a minute or two at the sun, it shortened generally ,24, ,26, and ,30 of an inch, in focal length.

The observations which are now before us, appear to be sufficient to establish the following principle; namely,

" That in order to see well with telescopes, it is required that " the temperature of the atmosphere and mirror should be uni- " form, and the air fraught with moisture."

This being admitted, we shall find no difficulty in accounting for every one of the foregoing observations.

If an uniform temperature be necessary, a frost after mild weather, or a thaw after frost, will derange the performance of our mirrors, till either the frost or the mild weather are sufficiently settled, that the temperature of the mirror may accommodate itself to that of the air. For, till such an uniformity with the open air, in the temperature of the mirror, the tube, the eye-glasses, and I would almost add the observer, be obtained, we cannot expect to see well. See observation 15, 17, 18, 19, and 23.

But, when a frost, though very severe, becomes settled, the mirror will soon accommodate itself to the temperature; and we shall find our telescopes to act well. See obs. 16, 20, 21, 22, 24, and 25.

This explains, with equal facility, why no telescope just brought out of a warm room can act properly. See obs. 40 and 41.

Nor can we ever expect to make a delicate observation, with high magnifying powers, when looking through a door, window, or slit in the roof of an observatory; even a confined place, though in the open air, will be detrimental. See obs. 42 and 43.

It equally shows, that windy weather in general, which must occasion a mixture of airs of different temperatures, cannot be favourable to distinct vision. See obs. 31, 32, and 33.

The same remark will apply to Auroræ Boreales, when they

induce, as they often do, a considerable change in the temperature of the different regions of air. See obs. 28.

But, should they not be accompanied by such a change, there seems to be no reason why they should injure vision. See obs. 29 and 30.

The warm exhalations from the roof of a house in a cold night, must disturb the uniformity of the temperature of a small portion of air; so that stars which are over the house, and at no considerable distance, may be affected by it. See obs. 38 and 39.

Sometimes the weather appears to be fine, and yet our telescopes will not act well. This may be owing to dryness occasioned by an easterly wind; or to a change of temperature, arising from an agitation of the upper regions of the atmosphere. See obs. 34 and 35.

Or, possibly, to both these causes combined together. See obs. 36 and 37.

If moisture in the atmosphere be necessary, dry air cannot be proper for vision. See obs. 26 and 27.

And therefore, on the contrary, dampness, and haziness of the atmosphere, must be favourable to distinct vision. See obs. 1, 2, 3, 4, 6, and 8.

Fogs also, which certainly denote abundance of moisture, must be very favourable to distinct vision. See obs. 9, 10, 11, 12, 13, and 14.

Nay, if the observatory should be surrounded by water, we need be under no apprehension on that account. Perhaps, were we to erect a building for astronomical purposes only, we ought not to object to grounds which are occasionally flooded; the neighbourhood of a river, a lake, or other generally called damp situations. See obs. 5 and 7.

It is however possible, that fogs and haziness may increase to such a degree as, at last, to take away, by their interposition, all the light which comes from celestial objects; in which case, they must of course put an end to observation; but they will nevertheless be accompanied with distinct vision to the very last. See obs. 44, 45, and 46.

We have now only the four last observations to account for. They relate to the change of the focal length of mirrors in solar observations, and its attendant derangement of the foci of the different parts of the reflecting surface; and, as simplicity is one of the marks of the truth of a principle, I believe we need not have recourse to any other cause than the change of temperature produced by the action of the solar rays that occasion heat; which will be quite sufficient to explain all the phenomena. But, in order to show this in its proper light, I shall relate the following experiments.

1st Experiment.

I placed a glass mirror, of 7-feet focal length, in the tube belonging to the telescope; and, having laid it open at the back, I prepared a stand, on which the iron used in my experiments on the terrestrial Rays that occasion Heat (see Phil. Trans. for 1800, Plate XVI. Fig. 1) might be placed, so as to heat the mirror from behind, while I kept a certain object in the field of view of the telescope. Having measured the focal length, and also [illegible] the figure of the mirror, which was parabolical, the heated iron was applied so as to be about 2½ inches from the back of the glass mirror. The consequence of this was, that [illegible] foci took place, so that the letters on a printed card in view, which before had been extremely distinct,

became instantly illegible. In 15 seconds, the focus of the mirror was shortened 2,3 inches; in half a minute, 3,47 inches; and, at the end of the minute, I found it no less than 4,59 inches shorter than it had been before the application of the hot iron.

On repeating the experiment, but placing the heated iron no more than $\frac{3}{4}$ of an inch from the back of the mirror, its focal length, in $1\frac{1}{2}$ minute, became 5,33 inches shorter.

I tried also a more moderate heat; and, placing the iron at 3 inches from the back, the focus of the mirror shortened in one minute 2,83 inches.

A thermometer placed in contact with the reflecting surface of the mirror, could hardly be perceived to have risen, during the time in which the hot iron produced the alteration of the focal length.

2d Experiment.

Every thing remaining as before, I suspended a small globe of heated iron in front of the mirror, at one inch and a half from its vertex; and, in two minutes, the focus was lengthened 5,3 inches. The figure of the mirror was also deranged; so that the letters on the card could not be distinguished.

I made a second trial, with the suspended iron a little more heated, and brought it as near the surface of the mirror as I judged it to be safe; since a contact would probably have cracked the mirror. In consequence of this arrangement, the focus lengthened, in one minute, 1,64 inch.

On removing the heated iron, the mirror returned, in one minute, to within ,18 inch of its former focal length; and, at the end of the second minute seemed to be nearly restored. But the disagreement of the foci of the different parts of the reflecting surface might be perceived for a long time afterwards,

and caused an indistinctness of vision, which plainly indicated that, under such circumstances, the magnifying power of the telescope, 225, was more than it ought to be, in order to see well.

3d Experiment.

I now changed the glass mirror for a metalline one; and, on placing the heater near the back of it, the focus of the speculum, in 30 seconds, became ,77 inch shorter. But, continuing the observation, instead of shortening still farther in the next 30 seconds, it became ,3 inch longer, so that, at the end of a minute, it was only ,47 shorter than before the approach of the hot iron.

4th Experiment.

When the small heated globe of the 2d experiment was suspended in front of the mirror, the focus lengthened ,27 inch in one minute; nor would the lengthening increase by leaving the hot iron longer in its position. The foci in this, as well as in the 3d experiment, were so much injured that they could not be measured with any precision; and it was evident, that high magnifying powers ought not to be used with a mirror of which the temperature is undergoing a continual change.

I repeated the experiment with the iron nearly red hot; and found the focus lengthened 1,48 inch in 30 seconds. Five minutes after the removal of the iron, the regularity of the figure of the mirror was pretty well restored.

With a moderate heat, I had, in 30 seconds, a lengthening of the focus, of ,57 inch; and, in about 1½ minute after the removal of the heated iron, distinct vision was nearly restored.

These four experiments show, that a change in the temperature of mirrors occasioned by heat, is attended with an alteration

of their focal length; and also prove, that the figure of the reflecting surface is considerably injured, during the time that such a change takes place. We are consequently authorised to believe, that the small alteration in the focus of a mirror exposed to the rays of the sun, arises from the same cause. For, since a thermometer, when the sun is shining upon it, will show that its temperature is altered, the action of the solar rays upon a mirror must be attended with a similar effect in its temperature. See obs. 47, 48, 49, and 50.

The same experiments will now also explain why the observations of the sun, related in our transit of Mercury, between $10^h\ 32'$ and $11^h\ 28'$, were not attended with success; for we have seen that heat occasions a derangement in the action of the reflecting surface; and it follows that, under such circumstances, high magnifying powers cannot be expected to show objects very distinctly.

If it should be remarked, that I have not explained why the focus of a glass mirror should shorten by the same rays of the sun which lengthen that of a metalline speculum, I confess that this at present does not appear; and, as it is not material to our purpose, I might pass it over in silence. We are however pretty well assured, that the alterations of the focal length must be owing to a dilatation of the glass or metal of which mirrors are made, and must be greatest where most heat is applied. Our experiments therefore cannot agree perfectly with solar observations; for, in the glass mirror, the application of partial heat in front, must undoubtedly have been much stronger about the middle of the mirror (though the centre of it was sometimes guarded by a brass plate equal to the size of the small speculum) than at the circumference. But when, on the contrary, a mirror

is exposed to the sun, every part of the surface will receive an equal portion of heat.

It may also be said, that I have pointed out a defect in telescopes used for solar observations, without assigning a cure for it. It will however be allowed, that tracing an evil to its cause must be the first step towards a remedy. Had the imperfection of the figure brought on by the heat of the solar rays been of a regular nature, an elliptical speculum might have been used to counteract the assumed hyperbolical form; or *vice versâ*.

And now, as, properly speaking, the derangement of the figure of a mirror used in observing the sun, is not so much caused by the heat of its rays as by their partial application to the reflecting surface only, which produces a greater dilatation in front than at the back, there may be a possibility of counteracting this effect, by a contrary application of heat against the back, or by an interception of it on the front. But this we leave to future experiments.

IX. *An Account of some Experiments and Observations on the constituent Parts of certain astringent Vegetables; and on their Operation in Tanning. By* Humphry Davy, *Esq. Professor of Chemistry in the Royal Institution. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read February 24, 1803.

The discovery made by M. Seguin, of a peculiar vegetable matter which is essential to the tanning of skin, and which is possessed of the property of precipitating gelatine from its solutions, has added considerably to our knowledge of the constituent parts of astringent vegetables.

Mr. Proust has investigated many of the properties of this substance; but, though his labours, and those of other chemists, have led to various interesting observations, yet they are far from having exhausted the subject. The affinities of tannin have been hitherto very little examined; and the manner in which its action upon animal matters is modified by combination with other substances, has been scarcely at all studied.

At the desire of the Managers of the Royal Institution, I began, in September, 1801, a series of experiments on the substances employed in the process of tanning, and on the chemical agencies concerned in it. These experiments have occupied, ever since, a considerable portion of my leisure hours; and I now presume to lay before the Royal Society an account of their general results. My chief design was to attempt to elucidate the

practical part of the art; but, in pursuing it, I was necessarily led to general chemical inquiries concerning the analysis of the different vegetable substances containing tannin, and their peculiar properties.

I. OBSERVATIONS ON THE ANALYSIS OF ASTRINGENT VEGETABLE INFUSIONS.

The substances that have been supposed to exist most generally in astringent infusions are, tannin, gallic acid, and extractive matter.

The presence of tannin in an infusion, is denoted by the precipitate it forms with the solution of glue, or of isinglass. And, when this principle is wholly separated, if the remaining liquor gives a dark colour with the oxygenated salts of iron, and an immediate precipitate with the solutions of alum and of muriate of tin, it is believed to contain gallic acid, and extractive matter.

The experiments of MM. Fourcroy, Vauquelin, and Seguin, have shown that many astringent solutions undergo a change by exposure to the atmosphere; an insoluble matter being precipitated from them. A precipitation is likewise occasioned in them by the action of heat; and these circumstances render it extremely difficult to ascertain, with any degree of precision, the quantities of their constituent parts, as they exist in the primitive combination.

After trying several experiments on different methods of ascertaining the quantity of tannin in astringent infusions, I was induced to employ the common process of precipitation by gelatine, as being the most accurate, [illegible]

This process, however, requires many precautions. The tannin [illegible] vegeta[illegible] [illegible]

the quantity of the precipitate obtained by filtration, is not always exactly proportional to the quantities of tannin and gelatine in solutions, but is influenced by the degree of their concentration. Thus, I found that 10 grains of dry isinglass, dissolved in two ounces of distilled water, gave, with solution of galls in excess, a precipitate weighing, when dry, 17 grains; whilst the same quantity, dissolved in six ounces of water, produced, all other circumstances being similar, not quite 15 grains. With more diluted solutions, the loss was still greater; and analogous effects took place, when equal portions of the same solution of isinglass were acted on by equal portions of the same infusion of galls diluted in different degrees with water; the least quantity of precipitate being always produced by the least concentrated liquor. In all cases, when the weak solutions were used, it was observed, that the residual fluid, though passed two or three times through the filtre, still remained more or less turbid and opaque; so that it is most likely that the deficiency arose from the continued suspension of some of the minutely divided solid matter in the liquid mass.

The solutions of gelatine, for the purposes of analysis, should be employed only when quite fresh, and in as high a state of saturation as is compatible with their perfect fluidity. I have observed, that in cases when they approach towards the state of jelly, their power of acting upon tannin is materially altered, and they produce only a very slight precipitation. As the degree of fluidity of solutions of gelatine is influenced by their temperature, I have found it expedient, in all comparative experiments, to bring them and the astringent infusions on which they are [illegible] nearly [illegible] degree of [illegible] temperat[illegible] 60

and 70° Fahrenheit; and the solutions of gelatine that I have used, were made by dissolving 120 grains of isinglass in 20 ounces of water.

In ascertaining the proportions of tannin in astringent infusions, great care must be taken to prevent the presence of any excess of gelatine; for, when this excess exists, I have found that a small portion of the solid compound formed is redissolved, and the results of the experiment otherwise affected. It is not difficult to discover the precise point of saturation, if the solution of isinglass be added only in small quantities at a time, and if portions of the clear liquor be passed through a filtre at different periods of the process. The properties of these portions will indicate the quantities of the solution of gelatine required for the completion of the experiment.

That the composition of any precipitate containing tannin and gelatine may be known with a tolerable degree of precision, it is necessary that the isinglass employed in the solution, and the new compound formed, be brought as nearly as possible to the same degree of dryness. For this purpose, I have generally exposed them, for an equal time, upon the lower plate of a sand-bath, which was seldom heated to more than 150°. This method I have found much better than that of drying at the temperatures of the atmosphere, as the different states of the air, with regard to moisture, materially influence the results.

Mr. Hatchett has noticed, in his excellent Paper on Zoophytes, [illegible] that isinglass is almost wholly composed of gelatine. I have found, that 100 grains of good and dry isinglass contain [illegible] of matter soluble in water. So that, [illegible] isinglass, [illegible] employed for

acting upon an astringent infusion, is compared with the quantity of the precipitate obtained, the difference between them will indicate the proportion of tannin, as it exists in the combination.

After the tannin has been separated from an astringent infusion, for the purpose of ascertaining its other component parts, I have been accustomed to evaporate the residual liquor very slowly, at a temperature below 200°.* In this process, if it contains extractive matter, that substance is in part rendered insoluble, so as to fall to the bottom of the vessel. When the fluid is reduced to a thick consistence, I pour alcohol upon it. If any gallic acid or soluble extractive matter be present, they will be dissolved, after a little agitation, in the alcohol; whilst the mucilage, if any exist, will remain unaltered, and may be separated from the insoluble extract, by lixiviation with water.

I have made many experiments, with the hope of discovering a method by which the respective quantities of gallic acid and extractive matter, when they exist in solution in the alcohol, may be ascertained; but without obtaining success in the results. It is impossible to render the whole of any quantity of extractive matter insoluble by exposure to heat and air, without at the same time decomposing a portion of the gallic acid. That acid cannot be sublimed, without being in part destroyed; and, at the temperature of its sublimation, extractive matter is wholly converted into new products.

Ether dissolves gallic acid; but it has comparatively little

* M. Deyeux [illegible] (*Annales de Chimie*, Tome XVII. page 36,) that in the process of evaporating [illegible] of galls, no gallic acid is carried over by the water, at a temperature below [illegible] ebullition. [illegible] infusions, however, [illegible] a portion of their [illegible], even in [illegible] are not [illegible] boil; but this [illegible] the smell [illegible] that [illegible] cannot be [illegible].

action upon extractive matter. I have been able, in examining solutions of galls, to separate a portion of gallic acid by means of ether. But, when the extractive matter is in large quantities, this method does not succeed, as, in consequence of that affinity which is connected with mass,* the greatest part of the acid continues to adhere to the extract.

Alumine has a strong attraction for extractive matter; but comparatively a weak one for gallic acid.† When carbonate of alumine is boiled for some time with a solution containing extractive matter, the extractive matter is wholly taken up by the earth, with which it forms an insoluble compound; but, into this compound, some of the gallic acid appears likewise to enter; and the portion remaining dissolved in the solution is always combined with alumine.

I have not, in any instance, been able to separate gallic acid and extractive matter perfectly from each other; but I have generally endeavoured to form some judgment concerning their relative proportions, by means of the action of the salts of alumine, and the oxygenated salts of iron. Muriate of alumine precipitates much of the extractive matter from solutions, without acting materially upon gallic acid; and, after this precipitation, some idea may be formed concerning the quantity of the gallic acid, by the colour it gives with the oxygenated sulphate of iron. In this process, however, great care must be taken not to add the solution of the sulphate of iron in excess; for, in this case, the black precipitate formed with the gallic acid will be redissolved, and a clear olive-coloured fluid only will be obtained.

The saline matters in astringent infusions, adhere so strongly to the vegetable principles, that it is impossible to ascertain their nature with any degree of accuracy, by means of common reagents. By incineration of the products obtained from the evaporation of astringent infusions, I have usually procured carbonate of lime and carbonate of potash.

In the different analyses, as will be seen from the results given in the following sections, I have attended chiefly to the proportions of the tanning principle, and of the principles precipitable by the salts of iron, as being most connected with practical applications.

With regard to the knowledge of the nature of the different substances, as they exist in the primitive astringent infusion, we can gain, by our artificial methods of examination, only very imperfect approximations. In acting upon them by reagents, we probably, in many cases, alter their nature; and very few of them only can be obtained in an uncombined state. The comparison, however, of the products of different experiments with each other, is always connected with some useful conclusions; and the accumulation of facts with regard to the subject, must finally tend to elucidate this obscure but most interesting part of chemistry.

II. EXPERIMENTS ON THE INFUSIONS OF GALLS.

I have been very much assisted in my inquiries concerning the properties of the infusions of galls, by the able Memoir of M. DEYEUX, on galls.*

The strongest infusion of galls that I could obtain, at 56°

FAHRENHEIT, by repeatedly pouring distilled water upon the best Aleppo galls broken into small pieces, and suffering it to remain in contact with them till the saturation was complete, was of the specific gravity 1.068. Four hundred grains of it produced, by evaporation at a temperature below 200°, fifty-three grains of solid matter; which, as well as I could estimate, by the methods of analysis that have been just described, consisted of about $\frac{9}{10}$ of tannin, or matter precipitable by gelatine, and $\frac{1}{10}$ of gallic acid, united to a minute portion of extractive matter.

100 grains of the solid matter obtained from the infusion, left, after incineration, nearly $4\frac{3}{4}$ grains of ashes; which were chiefly calcareous matter, mixed with a small portion of fixed alkali. The infusion strongly reddened paper tinged with litmus. It was semitransparent, and of a yellowish-brown colour. Its taste was highly astringent.

When sulphuric acid was poured into the infusion, a dense whitish precipitate was produced; and this effect was constant, whatever quantity of the acid was used. The residual liquor, when passed through the filtre, was found of a shade of colour deeper than before. It precipitated gelatine, and gave a dark colour with the oxygenated sulphate of iron.

The solid matter remaining on the filtre, slightly reddened vegetable blues; and, when dissolved in warm water, copiously precipitated the solutions of isinglass. M. PROUST,* who first paid attention to its properties, supposes that it is a compound of the acid with tannin; but I suspect that it also contains gallic acid, and probably a small portion of extractive matter.

* [illegible] of solution of galls by acids, was noticed by M. [illegible] Tome XX[illegible]

This last substance, as is well known, is thrown down from its solutions by sulphuric acid; and I found, in distilling the precipitate from galls by sulphuric acid, at a heat above 212°, that a fluid came over, of a light yellow colour, which was rendered black by oxygenated sulphate of iron; but which was not altered by gelatine.

Muriatic acid produced, in the infusion, effects analogous to those produced by sulphuric acid; and two compounds of the acid and the vegetable substances were formed: the one united to excess of acid, which remained in solution; the other containing a considerable quantity of tannin, which was precipitated in the solid form.

When concentrated nitric acid was made to act upon the infusion, it was rendered turbid; but the solid matter formed was immediately dissolved with effervescence, and the liquor then became clear, and of an orange colour. On examining it, it was found that both the tannin and the gallic acid were destroyed; for it gave no precipitate, either with gelatine or the salts of iron, even after the residual nitric acid was saturated by an alkali. By evaporation of a portion of the fluid, a soft substance was obtained, of a yellowish-brown colour, and of a slightly sourish taste. It was soluble in water, and precipitated the nitro-muriate of tin, and the nitrate of alumine; so that its properties approached to those of extractive matter; and it probably contained oxalic acid, as it rendered turbid a solution of muriate of lime.

When a very weak solution of nitric acid was mixed with the infusion, a permanent precipitate was formed; and the residual liquor, examined by the solution of gelatine, was found to contain tannin.

A solution of pure potash was poured into a portion of the

infusion. At first, a faint turbid appearance was perceived; but, by agitation, the fluid became clear, and its colour changed from yellow brown to brown red; and this last tint was most vivid on the surface, where the solution was exposed to the atmosphere. The solution of isinglass did not act upon the infusion modified by the alkali, till an acid was added in excess, when a copious precipitation was occasioned.

The compound of potash and solution of galls, when evaporated, appeared in the form of an olive-coloured mass, which had a faint alkaline taste, and which slowly deliquesced when exposed to the air.

Soda acted upon the infusion in the same manner as potash; and a fluid was formed, of a red-brown colour, which gave no precipitate to gelatine.

Solution of ammonia produced the same colour as potash and soda, and formed so perfect an union with the tannin of the infusion, that it was not acted upon by gelatine. When the compound liquor was exposed to the heat of boiling water, a part of the ammonia flew off, and another part reacted upon the infusion, so as to effect a material change in its properties. A considerable quantity of insoluble matter was formed; and the remaining liquor contained little tannin and gallic acid, but a considerable portion of a substance that precipitated muriate of tin, and the salts of alumine.

When the experiment on the ebullition of the compound of the infusion and ammonia was made in close vessels, the liquor that came over was strongly impregnated with ammonia; its colour [illegible]; and, when saturated with an acid, [illegible] by the salts of iron. The residual fluid, after the process had been continued for some time, as in the

other case, precipitated gelatine slightly, but the salts of alumine copiously; and it gave a tinge of red to litmus paper.

When solution of lime, of strontia, or of barytes, was poured in excess into a portion of the infusion, a copious olive-coloured precipitate was formed, and the solution became almost clear, and of a reddish tint. In this case, the tannin, the gallic acid, and the extractive matter, seemed to be almost wholly carried down in the precipitates: as the residual fluids, when saturated by an acid, gave no precipitate to gelatine, and only a very slight tint of purple to oxygenated sulphate of iron.

When the solutions of the alkaline earths were used only in small quantities, the infusion being in excess, a smaller quantity of precipitate was formed, and the residual liquor was of an olive-green colour; the tint being darkest in the experiment with the barytes, and lightest in that with the lime. This fluid, when examined, was found to hold in solution a compound of gallic acid and alkaline earth. It became turbid when acted on by a little sulphuric acid; and, after being filtrated, gave a black colour with the solutions of iron, but was not acted upon by gelatine.

When a large proportion of lime was heated for some time with the infusion, it combined with all its constituent principles, and gave, by washing, a fluid which had the taste of lime-water, and which held in solution only a very small quantity of vegetable matter. Its colour was pale yellow; and, when saturated with muriatic acid, it did not precipitate gelatine, and gave only a slight purple tinge to the solutions of the salts of iron. The lime in combination with the solid matter of the infusion, was of a fawn colour. It became green at its surface, where it was exposed to the air; and, when washed with large quantities of water,

it continued to give, even to the last portions, a pale yellow tinge.

Magnesia was boiled in one portion of the infusion for a few hours; and mixed in excess with another portion, which was suffered to remain cold. In both cases, a deep green fluid was obtained, which precipitated the salts of iron, but not the solutions of gelatine; and the magnesia had acquired a grayish-green tint. Water poured upon it became green, and acquired the properties of the fluid at first obtained. After long washing, the colour of the magnesia changed to dirty yellow; and the last portions of water made to act upon it were pale yellow, and altered very little the solutions of iron.

When the magnesia was dissolved in muriatic acid, a brownish and turbid fluid was obtained, which precipitated gelatine and the oxygenated salts of iron. So that there is every reason to believe, that the earth, in acting on the astringent infusion, had formed two combinations; one containing chiefly gallic acid, which was easily soluble in water; the other containing chiefly tannin, which was very difficultly soluble.

Alumine boiled with the infusion became yellowish-gray, and gave a clear white fluid, which produced only a tinge of light purple in the solutions of iron. When the earth* was employed [illegible] quantity, however, it formed an insoluble compound only with the tan[illegible] extract; and the residual liquor was found [illegible] with excess of acid.

The [illegible] of tin and [illegible] nitric acid, were boiled [illegible] of the [illegible] for two hours. In both [illegible] which appeared to be pure water, was

* [illegible] first observed the [illegible]. VAU-

obtained; and the oxides gained a tint of dull yellow. A part of each of them was dissolved in muriatic acid. The solution obtained was yellow: it copiously precipitated gelatine; and gave a dense black with the salts of iron. Mr. PROUST,* who first observed the action of oxide of tin upon astringent infusions, supposes that portions of tannin and gallic acid are decomposed in the process, or converted, by the oxygen of the oxide, into new substances. These experiments do not, however, appear to confirm the supposition.

M. DEYEUX observed, that a copious precipitation was occasioned in infusion of galls, by solutions of the alkalis combined with carbonic acid. Mr. PROUST has supposed that the solid matter formed is pure tannin, separated from its solution by the stronger affinity of the alkali for water; and he recommends the process, as a method of obtaining tannin.

In examining the precipitate obtained by carbonate of potash fully combined with carbonic acid, and used to saturation, I have not been able to recognise in it the properties which are usually ascribed to tannin: it is not possessed of the astringent taste; and it is but slightly soluble in cold water, or in alcohol. Its solution acts very little upon gelatine, till it is saturated with an acid; and it is not possessed of the property of tanning skin.

In various cases, in which the greatest care was taken to use no excess, either of the astringent infusion or of the alkaline solution, I have found the solid matter obtained possessed of analogous properties; and it has always given, by incineration, a considerable portion of carbonate of potash, and a small quantity of carbonate of lime.

The fluid remaining after the separation of the precipitate,

was of a dark-brown colour, and became green at the surface, when it was exposed to the air. It gave no precipitate to solution of gelatine; and afforded only an olive-coloured precipitate with the salts of iron.

When muriatic acid was poured into the clear fluid, a violent effervescence was produced; the fluid became turbid; a precipitate was deposited; and the residual liquor acted upon gelatine and the salts of iron, in a manner similar to the primitive infusion.

M. DEYEUX, in distilling the precipitate from infusion of galls by carbonate of potash, obtained crystals of gallic acid. In following his process, I had similar results; and a fluid came over, which reddened litmus-paper, and precipitated the salts of iron black, but did not act upon gelatine.

When the precipitate by carbonate of potash was acted upon by warm water, applied in large quantities, a considerable portion of it was dissolved; but a part remained, which could not in any way be made to enter into solution; and its properties were very different from those of the entire precipitate. It was not at all affected by alcohol: it was acted on by muriatic acid, and partially dissolved; and the solution precipitated gelatine and the salts of iron. It afforded, by incineration, a considerable portion of lime, but no alkali.

In comparing these facts, it would seem, that the precipitate from infusion of galls [illegible] partly of tannin and gallic acid united to [illegible] and partly of these vegetable matters [illegible] with [illegible] and it will [illegible] probable [illegible] hereafter [illegible] are examined, that both the [illegible] are contained in these compounds in a [illegible] carbonic acid. [illegible]

[illegible] solutions of carbonate of soda [illegible] carbonate of

ammonia, both precipitated the infusion of galls in a manner similar to the carbonate of potash; and each of the precipitates, when acted on by boiling water, left a small quantity of insoluble matter, which seemed to consist chiefly of tannin and carbonate of lime.

The entire precipitate by carbonate of soda produced, when incinerated, carbonate of soda and carbonate of lime. The precipitate by carbonate of ammonia, when exposed to a heat sufficient to boil water, in a retort having a receiver attached to it, gave out carbonate of ammonia, (which was condensed in small crystals in the neck of the retort,) and a yellowish fluid, which had the strong smell and taste of this volatile salt. After the process of distillation, the solid matter remaining was found of a dark brown colour; a part of it readily dissolved in cold water, and the solution acted on gelatine.

The residual fluid of the portions of the infusion which had been acted on by the carbonates of soda and of ammonia, as in the instance of the carbonate of potash, gave no precipitate with gelatine, till they were saturated with an acid; so that, in all these cases, the changes are strictly analogous.

The infusion of galls, as appears from the analysis, contains in its primitive state calcareous matter. By the action of the mild alkalis, this substance is precipitated in union with a portion of the vegetable matter, in the form of an insoluble compound. The alkalis themselves, at the same time, enter into actual combination with the remaining tannin and gallic acid; and a part of the compound formed is precipitated, whilst another part remains in solution.

When the [illegible] of lime, magnesia, and [illegible] were separately [illegible] with portions of the infusion of galls for

some hours, they combined with the tannin contained in it, so as to form with it insoluble compounds; and, in each case, a deep green fluid was obtained, which gave no precipitate to gelatine, even when an acid was added, but which produced a deep black colour in the solutions of the salts of iron.

Sulphate of lime, when finely divided, whether natural or artificial, after having been long heated with a small quantity of the infusion, was found to have combined with the tannin of it, and to have gained a faint tinge of light brown. The liquid became of a blue-green colour, and acted upon the salts of iron, but not upon gelatine; and there is every reason to suppose, that it held in solution a triple compound, of gallic acid, sulphuric acid, and lime.

We owe to Mr. Proust, the discovery that different solutions of the neutral salts precipitate the infusion of galls; and he supposes, that the precipitation is owing to their combining with a portion of the water which held the vegetable matter in solution. In examining the solid matters thrown down from the infusion, by sulphate of alumine, nitrate of potash, acetite of potash, muriate of soda, and muriate of barytes, I found them soluble, to a certain extent, in water, and possessed of the power of acting upon gelatine. From the products given by their incineration, and by their distillation, I am however inclined to believe that they contain, besides tannin, a portion of gallic acid and extractive matter, and a quantity of the salt employed in the primitive solution.

It is well known, that many of the metallic solutions occasion dense precipitates in the infusion of galls; and it has been generally supposed, that these precipitates are composed of tannin and extractive matter, or of those two substances and gallic acid, united

to the metallic oxide; but, from the observation of different processes of this kind, in which the salts of iron and of tin were employed, I am inclined to believe, that they contain also a portion of the acid of the saline compound.

When the muriate of tin was made to act upon a portion of the infusion, till no more precipitation could be produced in it, the fluid that passed through the filtre still acted upon gelatine, and seemed to contain no excess of acid; for it gave a precipitate to carbonate of potash, without producing effervescence. The solid compound, when decomposed by sulphuretted hydrogen, after the manner recommended by Mr. PROUST, was found strongly to redden litmus-paper, and it copiously precipitated nitrate of silver; whereas, the primitive infusion only rendered it slightly turbid; so that there is every reason to believe, that the precipitate contained muriatic acid.

By passing the [illegible]k and turbid fluid, procured by the action of solution of oxygenated sulphate of iron in excess [illegible] a portion of the infusion, through finely divided pure flint, contained in four folds of filtrating paper, [illegible] light olive-green fluid, in which there was no excess of [illegible] and which I [illegible] inclined to suppose, [illegible]s a solution of the compound of gallic acid and [illegible] of iron, with superabundance of metallic salt. I [illegible]y mentioned that gallic acid, when in very small proportion, does [illegible] the oxygenated salts of [illegible]nd Mr. P[illegible], in [illegible] Paper upon the Dif[illegible] Salts [illegible]ed that in the forma[illegible] [illegible] of [illegible] un[illegible] [illegible] of the [illegible]

conclude, that, in the case of the precipitation of tannin by the salts of tin and of iron, compounds are formed, of tannin and the salts; and that, of these compounds, such as contain tin are slightly soluble in water, whilst those that contain iron are almost wholly insoluble.

In examining the action of animal substances upon the infusion of galls, with the view of ascertaining the composition of the compounds of gelatine, and of skin, with tannin, I found that a saturated solution of gelatine, which contained the soluble matter of 50 grains of dry isinglass, produced from the infusion a precipitate that weighed nearly 91 grains; and, in another instance, a solution containing 90 grains of isinglass gave about 56 grains; so that, taking the mean of the two experiments, and allowing for the small quantity of insoluble matter in isinglass, we may conclude, that 100 grains of the compound of gelatine and tannin, formed by precipitation from saturated solutions, consist of about 54 grains of gelatine, and 46 of tannin.

A piece of dry calf-skin, perfectly free from extraneous matter, that weighed 180 grains, after being prepared for tanning by [illegible] water, was tanned in a portion of the infusion, being exposed to it for three weeks. When dry, the leather weighed [illegible] grains: so that, considering this experiment as [illegible], leather quickly tanned by means of an infusion of [illegible] of about [illegible] grains of skin, and [illegible] of vegetable

but I am inclined to attribute this effect, not to any absorption of gallic acid by the skin, but rather to the decomposition of it by the long continued action of the atmosphere; for much insoluble matter had been precipitated, during the process of tanning, and the residuum contained a small portion of acetous acid.

In ascertaining the quantity of tannin in galls, I found that 500 grains of good Aleppo galls gave, by lixiviation with pure water till their soluble parts were taken up, and subsequent slow evaporation, 185 grains of solid matter. And this matter, examined by analysis, appeared to consist,

Of tannin - - - - -	130 grains.
Of mucilage, and matter rendered insoluble by evaporation - - - - -	12
Of gallic acid, with a little extractive matter -	31
Remainder, calcareous earth and saline matter	12

The fluid obtained by the last lixiviation of galls, as M. Deyeux observed, is pale green; and I am inclined to believe, that it is chiefly a weak solution of gallate of lime. The ashes of galls, deprived of soluble matter, furnish a very considerable quantity of calcareous earth. And the property which M. Deyeux discovered in the liquor of the last lixiviations, of becoming red by the action of acids, and of regaining the green colour by means of alkalis, I have observed, more or less, in all

III. Experiments and observations on catechu or terra japonica.

The extract called catechu is said to be obtained from the wood of a species of the Mimosa,* which is found abundantly in India, by decoction and subsequent evaporation.

There are two kinds of this extract; one is sent from Bombay, the other from Bengal; and they differ from each other more in their external appearance than in their chemical composition. The extract from Bombay is of an uniform texture, and of a red-brown tint, its specific gravity being generally about 1.39. The extract from Bengal is more friable, and less consistent; its colour is like that of chocolate externally, but, when broken, its fracture presents streaks of chocolate and of red-brown. Its specific gravity is about [illegible]. Their tastes are precisely similar, being astringent, but leaving in the mouth a sensation of sweetness. They do not deliquesce, or apparently change, by exposure to the air.

The discovery of the tanning powers of catechu, is owing to the President of the Royal Society, who, concluding from its sensible properties that it contained tannin, furnished me, in December, 1802, with a quantity for chemical examination.

communicate this tinge to paper; they slightly redden litmus-paper; their taste is highly astringent, and they have no perceptible smell.

The strongest infusions that I could obtain from the two kinds of catechu, at 48° FAHRENHEIT, were of the same specific gravity, 1.057. But, by long decoction, I procured solutions of 1.102, which gave, by evaporation, more than $\frac{1}{6}$ of their weight of solid matter.

Five hundred grains of the strongest infusion of catechu from Bombay, furnished only 41 grains of solid matter; which, from analysis, appeared to consist of 34 grains of tannin, or matter precipitable by gelatine, and 7 grains that were chiefly a peculiar extractive matter, the properties of which will be hereafter described. The quantity of solid matter given by the strongest infusion of the Bengal catechu, was the same, and there was no [illegible] difference in its composition. Portions of these solid matters, when incinerated, left a residuum which seemed to be calcareous; but it was too small in quantity to be accurately examined, and it could not have [illegible] to more than $\frac{1}{100}$ of their original weights.

The strongest infusions of catechu acted upon the acids and pure alkalis in a manner analogous to the infusion of galls. With the concentrated sulphuric and muriatic acids, they gave dense light fawn coloured [illegible]. With strong nitrous ac[illegible] effervesced, and [illegible] power of precipitating the [illegible] the [illegible] iron. [illegible] al[illegible] [illegible] tannin, so as [illegible]

[illegible]

When [illegible]

poured into the infusions, copious precipitates, of a shade of light brown, were formed; and the residual fluid assumed a paler tint of red, and was found to have lost its power of precipitating gelatine.

After lime had been boiled for some time with a portion of the infusion, it assumed a dull red colour. The liquor that passed from it through the filtre had only a faint tint of red, did not act upon gelatine, and seemed to contain only a very small portion of vegetable matter. Pure magnesia, when heated with the infusion, acted upon it in an analogous manner; the magnesia became light red, and the residual fluid had only a very slight tinge of that colour. With carbonate of magnesia, the infusion became deeper in colour, and lost its power of precipitating gelatine; though it still gave, with oxygenated sulphate of [illegible] a light olive precipitate.

The carbonates of potash, of soda, and of ammonia, in their concentrated solutions, produced only a slight degree of turbidness in the infusion of catechu: they communicated to them a [illegible] deprived them of the power of acting upon [illegible] though this power was restored by the addition of an

The nitrate, or acetite, of lead, in concentrated solution, when poured into the infusion, produced in it a dense light brown precipitate, which gave to the fluid a gelatinous appearance. After this effect, there was no free acid found in it; and both the tannin and the extractive matter seemed to have been carried down, in union with a portion of the metallic salt.

The solution of muriate of tin, acted upon the infusion of catechu in a manner similar to that in which it acts upon the infusion of galls.

The least oxygenated sulphate of iron produced no change in the infusion. With the most oxygenated sulphate it gave a dense black precipitate, which, when diffused upon paper, appeared rather more inclined to olive than the precipitate from galls.

The infusions were precipitated by the solution of albumen.

The precipitates by gelatine had all a pale tint of red-brown, which became deeper when they were exposed to the air. The compound of gelatine and the tannin of the strongest infusions of catechu appeared, by estimation of the quantity of isinglass in the solutions used for their precipitation, to consist of about 41 parts of tannin, and 59 of gelatine.

Of two pieces of calf-skin which weighed, when dry, [illegible] grains each, and which had been prepared for tanning, one was immersed in a large quantity of the infusion of catechu from Bengal, and the other in an equal portion of the infusion of that from Bombay. In less [illegible] they were found con[illegible] leather. When fre[illegible] moisture, by long expos[illegible] in [illegible] they were weighed. The fir[illegible] had g[illegible] [illegible] the second piece 35½ [illegible]. The l[illegible] [illegible]

and on the upper surface was red-brown. It was not acted on by hot or cold water; and its apparent strength was the same as that of similar leather tanned in the usual manner.

In examining the remainder of the infusions of catechu, in which skin had been converted into leather, I found in them much less extractive matter than I had reason to expect, from the comparative analysis of equal portions of the unaltered infusions made by solutions of gelatine. At first, I was inclined to suppose that the deficiency arose from the action of the atmosphere upon the extractive matter, by which a part of it was rendered insoluble. But, on considering that there had been very little precipitation in the process, I was led to adopt the supposition, that it had entered into union with the skin, at the same time with the tannin; and this supposition was confirmed by new experiments.

[illegible] kinds of catechu are almost wholly soluble in large quantities of [illegible], and, to form a complete solution, about 18 ounces of water, at 52°, are required to 100 grains of extract. The residuum [illegible] amounts to $\frac{1}{14}$ of the original weight of [illegible] most cases, it is found to consist chiefly of [illegible] and aluminous earths, and of fine sand, which, by accident or design, had probably been mixed with the primitive

soluble in water than the tanning principle; and, when a small quantity of water is used to a large quantity of catechu, the quantity of tannin taken up, as appears from the nature of the strongest infusion, is very much greater than that of the extractive matter.

The extractive matter is much more soluble in warm water than in cold water; and, when saturated solutions of catechu are made in boiling water, a considerable quantity of extractive matter, in its pure state, falls down, as the liquor becomes cool.

The peculiar extractive matter of the catechu may be likewise obtained, by repeatedly lixiviating the catechu, when in fine powder, till the fluids obtained cease to precipitate gelatine; the residual solid will then be found to be the substance in question.

The pure [illegible] matter, whether [illegible] from the Bombay or Bengal catechu, is pale, with a faint tinge of red-brown. It has no perceptible smell; its [illegible] is slightly astringent; but it leaves in the mouth, for some [illegible] of sweetness, stronger than that given by the catechu itself.

Its solution in water is at first yellow-brown; but it gains a tint of red by exposure to the air. Its solution in alcohol does not materially change colour in the atmosphere; and it is of an uniform dull brown.

[illegible] matter, wh[illegible] or in solution, was not fou[illegible] any change of [illegible] upon [illegible] blue

The aqueous solution of it, when mixed with solutions of nitrate of alumine and of muriate of tin, became slightly turbid.

To nitrate of lead, it gave a dense light brown precipitate.

It was not perceptibly acted upon by solution of gelatine; but, when solution of sulphate of alumine was added to the mixture of the two fluids, a considerable quantity of solid matter, of a light brown colour, was immediately deposited.

To the solution of oxygenated sulphate of iron, it communicated a fine grass-green tint; and a green precipitate was deposited, which became black by exposure to the air.

It was not precipitated by the mineral acids.

Linen, by being boiled in the strongest solution of the extractive matter, acquired a light red-brown tint. The liquor became almost colourless; and, after this, produced very little change in the solution of oxygenated sulphate of iron.

Raw skin, prepared for tanning by being immersed in the strong solution, soon acquired the same kind of tint as the linen. It united itself to a part of the extractive matter; but it was not rendered by it insoluble in boiling water.

The solid extractive matter, when exposed to heat, softened, and became darker in its colour, but did not enter into fusion. At a temperature below that of ignition, it was decompounded. The volatile products of its decomposition were, carbonic acid hydrocarbonate, and water holding in solution acetous acid and a little unaltered extractive matter. There remained a light and very porous charcoal.

In considering the manner in which the catechu is prepared it would be reasonable to conclude, that different specimens of that substance must differ in some measure in their composition

even in their pure states; and, for the purposes of commerce, they are often adulterated to a considerable extent, with sand and earthy matter.*

In attempting to estimate the composition of the purest catechu, I selected pieces from different specimens, with which I was supplied by the President, and reduced them together into powder; mixing, however, only those pieces which were from catechu of the same kind.

Two hundred grains of the powder procured in this way, from the catechu of Bombay, afforded by analysis,

	Grains.
Tannin - - - - -	109
Peculiar extractive matter - - -	68
Mucilage - - - - -	13
Residual matter; chiefly sand and calcareous earth	10

The powder of the Bengal catechu gave, by similar methods of analysis, in 200 grains,

	Grains.
Tannin - - - - -	97
Peculiar extractive matter - - -	73
Mucilage - - - - -	16
Residual matter; sand, with a small quantity of calcareous and aluminous earths - - -	14

In examining those parts of the catechu from Bengal which were differently coloured, I found the largest proportion of tannin in the darkest part of the substance; and most extractive matter in the lightest part. It is probable that the inequality of composition in this catechu, is owing to its being evaporated

* One specimen that I examined, of the tetra japonica of commerce, furnished, by incineration, $\frac{1}{5}$ of sand and earthy matter; and another specimen, nearly $\frac{1}{7}$.

and formed without much agitation; in consequence of which, the constituent parts of it that are least soluble, being first precipitated, appear in some measure distinct from the more soluble parts, which assume the solid form at a later period of the process.

From the observations of Mr. KERR,* it would appear, that the pale catechu is that most sought after in India; and it is evidently that which contains most extractive matter. The extractive matter seems to be the substance that gives to the catechu the peculiar sweetness of taste which follows the impression of astringency; and it is probably this sweetness of taste which renders it so agreeable to the Hindoos, for the purpose of chewing with the betle-nut.

IV. EXPERIMENTS AND OBSERVATIONS ON THE ASTRINGENT INFUSIONS OF BARKS, AND OTHER VEGETABLE PRODUCTIONS.

The barks that I examined were furnished me by my friend SAMUEL PURKIS, Esq. of Brentford; they had been collected in the proper season, and preserved with care.

In making the infusions, I employed the barks in coarse powder; and, to expedite the solution, a heat of from 100 to 190° FAHRENHEIT was applied.

The strongest infusions of the barks of the oak, of the Leicester willow, and of the Spanish chesnut, were nearly of the same specific gravity, 1.05. Their tastes were alike, and strongly astringent; they all reddened litmus-paper; the infusion of the Spanish chesnut bark producing the highest tint; and that of the Leicester willow bark the feeblest tint.

Two hundred grains of each of the infusions were submitted

* Medical Observations, Vol. V. page 151.

to evaporation; and, in this process, the infusion of the oak bark furnished 17 grains of solid matter; that of the Leicester willow about $16\frac{1}{2}$ grains; and that of the Spanish chesnut nearly an equal quantity.

The tannin given by these solid matters was, in that from the oak bark infusion, 14 grains; in that from the willow bark infusion $14\frac{1}{2}$ grains; and in that from the Spanish chesnut bark infusion 13 grains.

The residual substances of the infusions of the Spanish chesnut bark, and of the oak bark, slightly reddened litmus-paper, and precipitated the solutions of tin of a fawn colour, and those of iron black. The residual matter of the infusion of the willow bark, did not perceptibly change the colour of litmus; but it precipitated the salts of iron of an olive colour, and rendered turbid the solution of nitrate of alumine.

The solid matters produced by the evaporation of the infusions, gave, by incineration, only a very small quantity of ashes, which could not have been more than $\frac{1}{160}$ of their original weights. These ashes chiefly consisted of calcareous earth and alkali; and the quantity was greatest from the infusion of chesnut bark.

The infusions were acted on by the acids, and the pure alkalis, in a manner very similar to the infusion of galls. With the solutions of carbonated alkalis, they gave dense fawn-coloured precipitates. They were copiously precipitated by the solutions of lime, of strontia, and of barytes; and, by lime-water in excess, the infusions of oak and of chesnut bark seemed to be deprived of the whole of the vegetable matter they held in solution.

By being boiled for some time with alumine, lime, and magnesia, they became almost colourless, and lost their power of

acting upon gelatine and the salts of iron. After being heated with carbonate of lime and carbonate of magnesia, they were found deeper coloured than before; and, though they had lost their power of acting on gelatine, they still gave dense olive-coloured precipitates with the salts of iron.

In all these cases, the earths gained tints of brown, more or less intense.

When the compound of the astringent principles of the infusion of oak bark with lime, procured by means of lime-water, was acted on by sulphuric acid, a solution was obtained, which precipitated gelatine, and contained a portion of the vegetable principles, and a certain quantity of sulphate of lime; a solid fawn-coloured matter was likewise formed, which appeared to be sulphate of lime, united to a little tannin and extractive matter.*

The solutions were copiously precipitated by solution of albumen.

The precipitates they gave with gelatine were similar in their appearance; their colour, at first, was a light tinge of brown, but they became very dark by exposure to the air. Their composition was very nearly similar; and, judging from the experiments on the quantity of gelatine employed in forming them, the compound of tannin and gelatine from the strongest infusion of oak bark, seems to consist, in the 100 parts, of 59 parts of

* M. MERAT GUILLOT proposes a method of procuring pure tannin, (*Annales de Chimie*, Tome [illegible] p. [illegible]) which consists in precipitating a solution of tan by [illegible], and decomposing it by nitric or muriatic acid. The solution of the solid matter obtained in this way in alcohol, he considers as a solution of pure tannin; but, from the experiments above-mentioned, it appears, that it must contain, besides tannin, some of the extractive matter of the bark; and it may likewise contain saline

gelatine and 41 of tannin; that from the infusion of Leicester willow bark, of 57 parts of gelatine and 43 of tannin; and that from the infusion of Spanish chesnut bark, of 61 parts of gelatine and 39 of tannin.

Two pieces of calf-skin, which weighed when dry 120 grains each, were tanned; one in the strongest infusion of Leicester willow bark, and the other in the strongest infusion of oak bark. The process was completed, in both instances, in less than a fortnight; when the weight of the leather formed by the tannin of the Leicester willow bark was found equal to 161 grains; and that of the leather formed by the infusion of oak bark was equal to 164 grains.

When pieces of skin were suffered to remain in small quantities of the infusions of the oak bark, and of the Leicester willow bark, till they were exhausted of their tanning principle, it was found, that though the residual liquors gave olive-coloured precipitates with the solutions of sulphate of iron, yet they were scarcely rendered turbid by solutions of muriate of tin; and there is every reason to suppose, that a portion of their extractive matter had been taken up with the tannin by the skin.

I attempted, in different modes, to obtain uncombined gallic acid from the solid matter produced by the evaporation of the barks, but without success. When portions of this solid matter were exposed to the degree of heat that is required for the production of gallic acid from Aleppo galls, no crystals were formed; and the fluid that came over gave only a brown colour to the solution of salts of iron, and was found to contain much acetous acid and empyreumatic oil.

When pure water was made to act, in successive portions, upon oak bark in coarse powder, till all its soluble parts were

taken up, the quantities of liquor last obtained, though they did not act much upon solution of gelatine, or perceptibly redden litmus-paper, produced a dense black with the solution of sulphate of iron: by evaporation, they furnished a brown matter, of which a part was rendered insoluble in water by the action of the atmosphere; and the part soluble in water was not in any degree taken up by sulphuric ether; so that, if it contained gallic acid, it was in a state of intimate union with extractive matter.

Two pieces of calf-skin, which weighed when dry 94 grains each, were slowly tanned; one by being exposed to a weak infusion of the Leicester willow bark, and the other by being acted upon by a weak infusion of oak bark. The process was completed in about three months; and it was found, that one piece of skin had gained in weight 14 grains, and the other piece about $16\frac{1}{2}$ grains. This increase is proportionally much less than that which took place in the experiment on the process of quick tanning. The colour of the pieces of leather was deeper than that of the pieces which had been quickly tanned; and, to judge from the properties of the residual liquors, more of the extractive matters of the barks had been combined with them.

The experiments of Mr. BIGGIN* have shown, that similar barks, when taken from trees at different seasons, differ as to the quantities of tannin they contain: and I have observed, that the proportions of the astringent principles in barks, vary considerably according as their age and size are different; besides, these proportions are often influenced by accidental circumstances; so that it is extremely difficult to ascertain their distinct relations to each other.

In every astringent bark, the interior white bark (that is, the

* Phil. Trans. for 1799, p. 299.

part next to the alburnum) contains the largest quantity of tannin. The proportion of extractive matter is generally greatest in the middle or coloured part: but the epidermis seldom furnishes either tannin or extractive matter.

The white cortical layers are comparatively most abundant in young trees; and hence their barks contain, in the same weight, a larger proportion of tannin than the barks of old trees. In barks of the same kind, but of different ages, which have been cut at the same season, the similar parts contain always very nearly the same quantities of astringent principles; and the interior layers afford about equal portions of tannin.

An ounce of the white cortical layers of old oak bark, furnished, by lixiviation and subsequent evaporation, 108 grains of solid matter; and, of this, 72 grains were tannin. An equal quantity of the white cortical layers of young oak produced 111 grains of solid matter, of which 77 were precipitated by gelatine.

An ounce of the interior part of the bark of the Spanish chesnut, gave 89 grains of solid matter, containing 63 grains of tannin.

The same quantity of the same part of the bark of the Leicester willow, produced 117 grains, of which 79 were tannin.

An ounce of the coloured or external cortical layers from the oak, produced 43 grains of solid matter, of which 19 were tannin.

From the Spanish chesnut, 41 grains, of which 14 were tannin.

And, from the Leicester willow, 34 grains, of which 16 were tannin.

In attempting to ascertain the relative quantities of tannin in the different *entire* barks, I selected those specimens which appeared similar with regard to the proportions of the external

and internal layers, and which were about the average thickness of the barks commonly used in tanning, namely, $\frac{1}{2}$ an inch.

Of these barks, the oak produced, in the quantity of an ounce, 61 grains of matter dissolved by water, of which 29 grains were tannin.

The Spanish chesnut 53 grains, of which 21 were tannin.

And the Leicester willow 71 grains, of which 33 were tannin.

The proportions of these quantities, in respect to the tanning principle, are not very different from those estimated in Mr. BIGGIN's table.*

The residual substances obtained in the different experiments, differed considerably in their properties; but certain portions of them were, in all instances, rendered insoluble during the process of evaporation. The residuum of the chesnut bark, as in the instance of the strongest infusion, possessed slightly acid properties; but more than $\frac{3}{4}$ of its weight consisted of extractive matter. All the residuums in solution, as in the other cases, were precipitated by muriate of tin; and, after this precipitation, the clear fluids acted much more feebly than before on the salts of iron; so that there is great reason for believing, that the power of astringent infusions to precipitate the salts of iron black, or dark coloured, depends partly upon the agency of the extractive matters they contain, as well as upon that of the tanning principle and gallic acid.

In pursuing the experiments upon the different astringent infusions, I examined the infusions of the bark of the elm and of the common willow. These infusions were acted on by reagents, in a manner exactly similar to the infusions of the other barks: they were precipitated by the acids, by solutions of the

* Phil. Trans. for 1799, p. 263.

alkaline earths, and of the carbonated alkalis; and they formed, with the caustic alkalis, fluids not precipitable by gelatine.

An ounce of the bark of the elm, furnished 13 grains of tannin.

The same quantity of the bark of the common willow, gave 11 grains.

The residual matter of the bark of the elm, contained a considerable portion of mucilage; and that of the bark of the willow, a small quantity of bitter principle.

The strongest infusions of the sumachs from Sicily and Malaga, agree with the infusions of barks, in most of their properties; but they differ from all the other astringent infusions that have been mentioned, in one respect; they give dense precipitates with the caustic alkalis. Mr. Proust has shown, that sumach contains abundance of sulphate of lime; and it is probably to this substance that the peculiar effect is owing.

From an ounce of Sicilian sumach, I obtained 165 grains of matter soluble in water, and, of this matter, 78 grains were tannin.

An ounce of Malaga sumach, produced 156 grains of soluble matter, of which 79 appeared to be tannin.

The infusion of Myrobalans* from the East Indies, differed from the other astringent infusions chiefly by this circumstance, that it effervesced with the carbonated alkalis; and it gave with them a dense precipitate, that was almost immediately redissolved. After the tannin had been precipitated from it by gelatine, it strongly reddened litmus-paper, and gave a bright black with the solutions of iron. I expected to be able to procure

* The Myrobalans used in these experiments are the fruit of the Terminalia Chebula. Retz. *Obs. Botan.* Fasc. V. p. 31.

gallic acid, by distillation, from the Myrobalans; but in this I was mistaken; they furnished only a pale yellow fluid, which gave merely a slight olive tinge to solution of sulphate of iron.

Skin was speedily tanned in the infusion of the Myrobalans; and the appearance of the leather was similar to the appearance of that from galls.

The strongest infusions of the teas are very similar, in their agencies upon chemical tests, to the infusions of catechu.

An ounce of Souchong tea, produced 48 grains of tannin.

The same quantity of green tea, gave 41 grains.

Dr. MATON has observed, that very little tannin is found in cinchona, or in the other barks supposed to be possessed of febrifuge properties. My experiments tend to confirm the observation. None of the infusions of the strongly bitter vegetable substances that I have examined, give any precipitate to gelatine. And the infusions of quassia, of gentian, of hops, and of chamomile, are scarcely affected by muriate of tin; so that they likewise contain very little extractive matter.

In all substances possessed of the astringent taste, there is great reason to suspect the presence of tannin; it even exists in substances which contain sugar and vegetable acids. I have found it in abundance in the juice of sloes; and my friend Mr. POOLE, of Stowey, has detected it in port wine.

V. GENERAL OBSERVATIONS.

Mr. PROUST has supposed, in his Paper upon Tannin and its Species,* that there exist different species of the tanning principle, possessed of different properties, and different powers

* *Annales de Chimie*, Tome XLI, p. 332.

of acting upon reagents, but all precipitable by gelatine. This opinion is sufficiently conformable to the facts generally known concerning the nature of the substances which are produced in organised matter; but it cannot be considered as proved, till the tannin in different vegetables has been examined in its pure or insulated state. In all the vegetable infusions which have been subjected to experiment, it exists in a state of union with other principles; and its properties must necessarily be modified by the peculiar circumstances of its combination.

From the experiments that have been detailed it appears, that the *specific* agencies of tannin in all the different astringent infusions are the same. In every instance, it is capable of entering into union with the acids, alkalis, and earths; and of forming insoluble compounds with gelatine, and with skin. The infusions of the barks affect the greater number of reagents in a manner similar to the infusion of galls; and, that this last fluid is rendered green by the carbonated alkalis, evidently depends upon the large proportion of gallic acid it contains. The infusion of sumach owes its characteristic property, of being precipitated by the caustic alkalis, to the presence of sulphate of lime; and, that the solutions of catechu do not copiously precipitate the carbonated alkalis, appears to depend upon their containing tannin in a peculiar state of union with extractive matter, and uncombined with gallic acid or earthy salts.

In making some experiments upon the affinities of the tanning principle, I found that all the earths were capable of attracting it from the alkalis; and, so great is their tendency to combine with it, that, by means of them, the compound of tannin and gelatine may be decomposed without much difficulty; for, after pure magnesia had been boiled for a few hours with

this substance diffused through water, it became of a red-brown colour, and the fluid obtained by filtration produced a distinct precipitate with solution of galls. The acids have less affinity for tannin than for gelatine; and, in cases where compounds of the acids and tannin are acted on by solution of gelatine, an equilibrium of affinity is established, in consequence of which, by far the greatest quantity of tannin is carried down in the insoluble combination. The different neutral salts have, comparatively, feeble powers of attraction for the tanning principle; but, that the precipitation they occasion in astringent solutions, is not simply owing to the circumstance of their uniting to a portion of the water which held the vegetable substances in solution, is evident from many facts, besides those which have been already stated. The solutions of alum, and of some other salts which are less soluble in water than tannin, produce, in many astringent infusions, precipitates as copious as the more soluble saline matters; and sulphate of lime, and other earthy neutral compounds, which are, comparatively speaking, insoluble in water, speedily deprive them of their tanning principle.

From the different facts that have been stated, it is evident that tannin may exist in a state of combination in different substances, in which its presence cannot be made evident by means of solution of gelatine; and, in this case, to detect its existence, it is necessary to have recourse to the action of the diluted acids.

In considering the relations of the different facts that have been detailed, to the processes of tanning and of leather-making, it will appear sufficiently evident, that when skin is tanned in astringent infusions that contain, as well as tannin, extractive matters, portions of these matters enter, with the tannin, into

chemical combination with the skin. In no case is there any reason to believe that gallic acid is absorbed in this process; and M. SEGUIN's ingenious theory of the agency of this substance, in producing the deoxygenation o skin, seems supported by no proofs. Even in the formation of glue from skin, there is no evidence which ought to induce us to suppose that it loses a portion of oxygen; and the effect appears to be owing merely to the separation of the gelatine, from the small quantity of albumen with which it was combined in the organised form, by the solvent powers of water.

The different qualities of leather made with the same kind of skin, seem to depend very much upon the different quantities of extractive matter it contains. The leather obtained by means of infusion of galls, is generally found harder, and more liable to crack, than the leather obtained from the infusions of barks; and, in all cases, it contains a much larger proportion of tannin, and a smaller proportion of extractive matter.

When skin is very slowly tanned in weak solutions of the barks, or of catechu, it combines with a considerable proportion of extractive matter; and, in these cases, though the increase of weight of the skin is comparatively small, yet it is rendered perfectly insoluble in water; and is found soft, and at the same time strong.

The saturated astringent infusions of barks contain much less extractive matter, in proportion to their tannin, than the weak infusions; and, when skin is quickly tanned in them, common experience shows that it produces leather less durable than the leather slowly formed.

Besides, in the case of quick tanning by means of infusions of barks, a quantity of vegetable extractive matter is lost to the

manufacturer, which might have been made to enter into the composition of his leather. These observations show, that there is some foundation for the vulgar opinion of workmen, concerning what is technically called the *feeding* of leather in the slow method of tanning; and, though the processes of the art may in some cases be protracted for an unnecessary length of time, yet, in general, they appear to have arrived, in consequence of repeated practical experiments, at a degree of perfection which cannot be very far extended by means of any elucidations of theory that have as yet been made known.

On the first view it appears singular that, in those cases of tanning where extractive matter forms a certain portion of the leather, the increase of weight is less than when the skin is combined with pure tannin; but the fact is easily accounted for, when we consider that the attraction of skin for tannin must be probably weakened by its union with extractive matter; and, whether we suppose that the tannin and extractive matter enter together into combination with the matter of skin, or unite with separate portions of it, still, in either case, the primary attraction of tannin for skin must be, to a certain extent, diminished.

In examining astringent vegetables in relation to their powers of tanning skin, it is necessary to take into account, not only the quantity they contain of the *substance* precipitable by gelatine, but likewise the quantity, and the nature, of the extractive matter; and, in cases of comparison, it is essential to employ infusions of the same degree of concentration.

It is evident, from the experiments detailed in the IIId section, that of all the astringent substances which have been as yet examined, catechu is that which contains the largest proportion of tannin; and, in supposing, according to the common

estimation, that from four to five pounds of common oak bark are required to produce one pound of leather, it appears, from the various synthetical experiments, that about half a pound of catechu would answer the same purpose.*

Also, allowing for the difference in the composition of the different kinds of leather, it appears, from the general detail of facts, that one pound of catechu, for the common uses of the tanner, would be nearly equal in value to $2\frac{1}{4}$ pounds of galls, to $7\frac{1}{2}$ pounds of the bark of the Leicester willow, to 11 pounds of the bark of the Spanish chesnut, to 18 pounds of the bark of the elm, to 21 pounds of the bark of the common willow, and to 3 pounds of sumach.

Various menstruums have been proposed for the purpose of expediting and improving the process of tanning, and, amongst them, lime-water and the solutions of pearl-ash: but, as these two substances form compounds with tannin which are not decomposable by gelatine, it follows that their effects must be highly pernicious; and there is very little reason to suppose, that any bodies will be found which, at the same time that they increase the solubility of tannin in water, will not likewise diminish its attraction for skin.

* This estimation agrees very well with the experiments lately made by Mr. PURKIS, upon the tanning powers of Bombay catechu in the processes of manufacture, and which he has permitted me to mention. Mr. PURKIS found, by the results of different accurate experiments, that one pound of catechu was equivalent to seven or eight of oak bark.

X. *Appendix to Mr.* William Henry's *Paper, on the Quantity of Gases absorbed by Water, at different Temperatures, and under different Pressures.* (See Page 29).

SINCE my Paper was printed, I have found that the numbers assigned in it, as indicating the quantities taken up, by water, of some of the more absorbable, and of all the less absorbable gases, are rather below the truth. The accuracy of these numbers I was led to doubt, by a suspicion that due attention had not always been paid, in my former experiments, to the quality of the unabsorbed residuum. For, the theory which Mr. DALTON has suggested to me on this subject, and which appears to be confirmed by my experiments, is, that the absorption of gases by water is purely a mechanical effect, and that its amount is exactly proportional to the density of the gas, considered abstractedly from any other gas with which it may accidentally be mixed. Conformably to this theory, if the residuary gas contain $\frac{1}{2}$, $\frac{1}{10}$, or any other proportion, of foreign gas, the quantity absorbed by water will be $\frac{1}{2}$, $\frac{1}{10}$, &c. short of the maximum. The proof of these propositions would lead me into a minuteness of detail, not suited to the present occasion; I therefore hasten to communicate the results of my latest experiments.

The report which I have already given, of the quantity of CARBONIC ACID GAS, absorbed under the ordinary pressure of the atmosphere, I find no reason to correct; but, of SULPHURETTED HYDROGEN GAS, I have effected a larger absorption

than the one before stated; and have repeatedly observed its amount to be 106 or 108 by 100 measures of water, at 60° of FAHRENHEIT, which temperature is to be understood in all the following experiments.

Of several experiments on the absorption of NITROUS OXIDE, I take the following, as a fair example of the whole. I agitated, at three several times, 1175 measures of nitrous oxide, with 1320 measures of water; 1025 parts of gas disappeared, and the unabsorbed remainder (150) contained 15 of foreign admixtures. It follows, that 100 parts of water had taken up 77.6 of nitrous oxide; and, after adding to these the diminution of absorption occasioned by the impurity of the residuum, it may be inferred, that 100 parts of water would absorb 86 of absolutely pure nitrous oxide.

With respect to the remaining gases, I have been prevented, by urgent professional engagements, from examining the quantity of each, absorbable under similar circumstances, except in the instances of oxygenous, azotic, and hydrogenous gases. The results of these experiments are comprised in the following Table. The first column shows the quantity of gas which 100 parts of water, at 60°, have actually absorbed; the second, the quantity which ought to be taken up, provided the residue were in a state of absolute purity. In the example of nitrous gas alone, the estimated is less than the actual absorption; because a small portion of this gas loses its aërial form, by union with the oxygenous gas, from which water cannot be entirely freed.

TABLE shewing the Quantity of each Gas absorbed by 100 measures of water, at 60°.

	Actual Absorption.	Inferred Absorption.
Nitrous gas - - -	5 -	5
Oxygenous gas - -	3.55 -	3.7
Phosphuretted hydrogen gas	2.14	
Gaseous oxide of carbon -	2.01	
Carburetted hydrogen gas -	1.40	
Azotic gas - - -	1.47 -	1.53
Hydrogenous gas - -	1.53 -	1.61

METEOROLOGICAL JOURNAL,

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PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1802.

1802	Six's Therm. least and greatest Heat.	Time. H	Time. M	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds Points.	Winds Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Jan. 1	26	8	0	27	48	30,01	72		NW	2	Fine.
	33	2	0	33	51	30,04	69		NW	2	Fine.
2	25	8	0	30	47	29,85	78		S	1	Hazy.
	35	2	0	35	50	29,68	83		S	2	Rain.
3	28	8	0	28	46	29,82	82		E	2	Snow.
	29	2	0	29	49	30,01	80		E	2	Hazy.
4	25	8	0	30	44	30,15	77		NE	2	Cloudy.
	33	2	0	33	48	30,04	79		NE	2	Cloudy.
5	30	8	0	31	44	29,56	83		NNW	2	Cloudy.
	33	2	0	33	46	29,50	83		NW	1	Snow.
6	31	8	0	31	44	29,48	85		S	1	Cloudy.
	35	2	0	35	47	29,55	85		SE	1	Snow.
7	24	8	0	24	43	29,77	82		NE	1	Fair.
	33	2	0	33	47	29,78	82		NE	1	Cloudy.
8	31	8	0	35	44	29,77	87		NE	1	Cloudy.
	36	2	0	36	47	29,80	86		NE	1	Rain.
9	34	8	0	34	45	29,83	85	0,060	NE	1	Cloudy.
	35	2	0	35	48	29,80	76		NE	1	Cloudy.
10	31	8	0	31	45	29,40	80		WNW	1	Cloudy.
	30	2	0	30	48	29,31	77		NW	1	Cloudy.
11	24	8	0	24	44	29,38	82		NNE	1	Fine.
	31	2	0	31	48	29,37	75		NW	1	Fine.
12	22	8	0	22	43	29,55	80		W	1	Cloudy.
	28	2	0	28	45	29,54	78		SW	1	Fair.
13	18	8	0	20	42	29,53	78		W	1	Snow.
	28	2	0	28	44	29,54	78		NW	1	Cloudy.
14	23	8	0	24	41	29,86	78		NW	1	Cloudy.
	30	2	0	30	45	29,88	75		NW	1	Fine.
15	22	8	0	24	41	30,21	82		NE	1	Cloudy.
	27	2	0	27	44	30,32	77		NE	1	Fair.
16	15	8	0	16	41	30,44	80		SW	1	Cloudy.
	34	2	0	34	44	30,35	80		SW	1	Fair.

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for January, 1802.

1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Jan. 17	33	8	0	37	42	30,17	88		SW	1	Cloudy.
	40	2	0	40	45	30,12	86		SW	1	Fair.
18	37	8	0	38	43	30,10	82		SW	1	Cloudy.
	44	2	0	44	47	30,08	75		SW	1	Cloudy.
19	39	8	0	41	45	29,86	85		SW	2	Cloudy.
	46	2	0	46	48	29,65	87		SSW	2	Cloudy.
20	35	8	0	36	45	29,68	79	0,070	WNW	2	Fine.
	46	2	0	42	49	29,84	71		WNW	2	Fair.
21	39	8	0	43	48	29,33	73		WSW	2	Rain. [Much wind last night.
	43	2	0	43	51	29,31	64		W	2	Fair.
22	37	8	0	37	48	30,00	71		WNW	2	Fine.
	41	2	0	41	52	30,15	68		NW	2	Fine.
23	30	8	0	32	48	30,44	81		SW	1	Hazy.
	41	2	0	41	51	30,44	76		SSW	1	Hazy.
24	38	8	0	40	49	30,29	85		SW	1	Rain.
	44	2	0	43	51	30,27	85		SW	1	Cloudy.
25	34	8	0	35	48	30,20	82	0,016	WSW	1	Fair.
	44	2	0	44	52	30,18	76		SW	1	Fine.
26	36	8	0	43	50	30,18	81		SSW	1	Cloudy.
	46	2	0	45	53	30,18	79		S	1	Fair.
27	40	8	0	44	51	30,35	89		S	2	Cloudy.
	48	2	0	48	54	30,42	88		S	1	Cloudy.
28	38	8	0	39	49	30,46	89		S	1	Fine.
	48	2	0	48	53	30,43	72		S	1	Fine.
29	38	8	0	40	51	30,25	85		S	2	Cloudy.
	44	2	0	44	53	30,16	86		S	2	Cloudy.
30	39	8	0	40	52	30,18	90		S	1	Cloudy.
	47	2	0	47	54	30,14	83		S	1	Cloudy.
31	45	8	0	46	52	30,08	86		S	2	Cloudy.
	48	2	0	48	53	30,02	81		S	2	Cloudy.

METEOROLOGICAL JOURNAL

for February, 1802.

1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points	Winds. Str	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 1	48	7	0	48	53	29,57	90	0,073	S	2	Cloudy.
	49	2	0	49	54	29,58	82		S	2	Cloudy.
2	41	7	0	41	53	29,61	83	0,175	SW	2	Fair.
	49	2	0	49	56	29,76	73		SW	2	Fair.
3	38	7	0	41	54	29,85	84		S	1	Cloudy.
	47	2	0	47	55	29,81	78		S	2	Cloudy.
4	36	7	0	40	52	29,80	82	0,072	S	2	Cloudy.
	46	2	0	46	53	29 65	73		S	2	Fair.
5	34	7	0	34	53	29,95	80	0,030	S	1	Fine.
	41	2	0	41	55	30,01	80		S	1	Fine.
6	39	7	0	43	53	29,28	90	0,158	S	2	Rain. [Much wind last night
	48	2	0	47	55	29,30	82		W	1	Cloudy.
7	37	7	0	37	51	29,84	85		NE	2	Cloudy.
	41	2	0	41	53	29 98	78		NE	2	Fair.
8	30	7	0	31	51	30,16	81		SSW	1	Hazy.
	41	2	0	41	53	29,96	78		S	2	Cloudy.
9	33	7	0	33	51	29,82	82	0,270	SW	1	Fair.
	41	2	0	42	53	29,82	70		WSW	1	Hazy.
10	32	7	0	32	50	29,58	84		SW	1	Fair.
	41	2	0	41	53	29,54	72		W	2	Fair.
11	32	7	0	34	50	29,55	82		NW	1	Snow.
	37	2	0	37	51	29 67	82		NE	1	Cloudy.
12	31	7	0	32	50	29,95	83		NE	1	Cloudy.
	39	2	0	39	52	30,02	77		NE	2	Fair.
13	30	7	0	31	50	30,14	82		NE	1	Cloudy.
	38	2	0	38	52	30,13	78		NE	1	Cloudy.
14	32	7	0	32	48	30,12	76		NE	1	Cloudy.
	36	2	0	36	49	30,10	74		NE	1	Cloudy.
15	31	7	0	31	48	29,92	78		NE	1	Cloudy.
	35	2	0	35	49	29,85	72		NE	1	Cloudy.
16	29	7	0	30	48	29 66	82		NE	1	Snow.
	38	2	0	38	50	29 61	80		W	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm within	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches	°	Inches.			
Feb. 17	32	7	0	36	48	29 41	·5		SSW	1	Cloudy.
	45	2	0	45	51	29,22	85		SSE	1	Cloudy.
18	38	7	0	38	48	29 19	87	0,085	EN	1	Cloudy.
	43	2	0	43	51	29,34	79		NE	1	Cloudy.
19	37	7	0	38	50	29,64	83		SW	1	Cloudy.
	45	2	0	45	53	29,56	80		S	1	Cloudy.
20	41	7	0	41	51	29·35	84	0,210	SW	2	Fair.
	47	2	0	47	53	29,50	80		NW	2	Cloudy.
21	43	7	0	46	52	29,50	90	0,078	SW	1	Cloudy.
	53	2	0	53	54	29,57	83		W	1	Cloudy.
22	49	7	0	51	53	29,69	86	0,052	WSW	1	Cloudy.
	55	2	0	55	57	29,76	83		WSW	1	Cloudy.
23	50	7	0	50	56	30,00	90	0,097	WSW	1	Cloudy.
	56	2	0	55	56	30,00	86		SW	1	Cloudy.
24	42	7	0	42	56	30,03	83		WSW	1	Fair.
	53	2	0	53	57	29,89	83		SW	1	Cloudy.
25	40	7	0	41	55	29,48	84	0,200	NE	2	Rain.
	48	2	0	48	57	29,85	74		NE	2	Fair.
26	34	7	0	34	54	30,16	79		NNE	1	Fair.
	43	2	0	43	56	30,14	72		SE	1	Hazy.
27	35	7	0	35	54	30,08	80		SSW	1	Fine.
	49	2	0	49	55	30,01	75		SW	1	Fair.
28	42	7	0	42	53	29,90	80		SW	1	Cloudy.
	47	2	0	47	55	29,84	76		SW	1	Cloudy.

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1802	Six's Therm least and greatest Heat	Time. H. M.		Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points	Winds. Str.	Weather.
	°			°	°		°				
Mar. 1	38	7	0	39	53	29,68	81		E	1	Cloudy.
	48	2	0	48	55	29.58	83		E	1	Cloudy.
2	45	7	0	45	54	29 52	86	0,062	E	1	Cloudy.
	53	2	0	53	57	29.47	83		SSW	1	Cloudy.
3	40	7	0	41	54	29,54	84	0,147	SW	1	Cloudy.
	48	2	0	48	56	29 [illegible]	75		SW	1	Cloudy.
4	37	7	0	37	54	29,68	80	0,016	NE	1	Cloudy.
	44	2	0	44	56	29 81	69		NE	1	Fair.
5	31	7	0	31	54	30,08	78		WSW	1	Fair.
	41	2	0	41	55	30,13	73		NE	1	Cloudy.
6	31	7	0	31	54	30 27	79		NE	1	Fine.
	41	2	0	41	56	30,26	68		NE	1	Fair.
7	29	7	0	29	52	30,21	78		NE	1	Fine.
	41	2	0	41	55	30,08	71		NE	1	Fine.
8	32	7	0	33	52	29,80	82		NE	1	Foggy.
	41	2	0	41	53	29.68	78		N	1	Cloudy.
9	34	7	0	34	51	29.94	73		NE	1	Cloudy.
	44	2	0	44	55	30,08	62		NE	2	Fine.
10	33	7	0	35	53	30,28	79		W	1	Cloudy.
	52	2	0	52	55	30.26	69		W	1	Cloudy.
11	42	7	0	42	54	30,25	82		W	1	Cloudy.
	56	2	0	56	56	30,17	68		W	1	Fair.
12	38	7	0	43	55	30,00	74		SW	1	Cloudy.
	52	2	0	52	55	29 91	70		SW	1	Cloudy.
13	35	7	0	36	54	30,00	72		WNW	2	Fine.
	42	2	0	42	55	30,06	66		NW	2	Fair.
14	30	7	0	30	52	30,32	68		NNE	2	Fine.
	41	2	0	41	54	30,36	66		NNE	2	Fair.
15	30	7	0	32	52	30,45	77		NE	2	Fair.
	43	2	0	43	55	30,40	65		NE	1	Fine.
16	27	7	0	29	51	30,30	78		W	1	Hazy.
	49	2	0	49	54	30,31	67		NNE	1	Fair.

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1802	Six's Therm least and greatest Heat.	Time H	Time M	Therm. without.	Therm. within	Barom.	Hygrometer.	Rain.	Winds. Points	Winds. Str	Weather.
	°			°	°	Inches.	°	Inches.			
Mar 17	32	7	0	34	52	30,44	77		WNW	1	Fair.
	51	2	0	51	53	30,41	68		NW	1	Cloudy.
18	36	7	0	37	53	30,28	60		W	1	Fine.
	55	2	0	55	56	30,20	71		SSW	1	Fine.
19	35	7	0	37	53	29,87	81		WNW	1	Cloudy.
	48	2	0	48	55	29,78	67		SW	2	Cloudy.
20	39	7	0	39	55	29,62	77	0,040	WSW	2	Fair.
	49	2	0	49	57	29,76	60		NW	[illegible]	Fair.
21	39	7	0	43	54	29,36	78		S	2	Cloudy.
	46	2	0	46	55	29,45	81		S	2	Cloudy.
22	39	7	0	41	54	29,47	86	0,048	WSW	1	Cloudy.
	50	2	0	50	56	29,32	76		W	1	Cloudy.
23	37	7	0	38	54	29,95	73	0,068	SW	1	Fine.
	51	2	0	49	55	30,04	67		WSW	1	Cloudy.
24	43	7	0	45	54	30,11	85	0,016	SW	1	Cloudy.
	60	2	0	59	57	30,18	80		SW	1	Cloudy.
25	46	7	0	46	56	30,36	90		SW	1	Cloudy.
	60	2	0	60	58	30,39	78		SW	1	Cloudy.
26	42	7	0	43	56	30,43	86		SW	1	Foggy.
	61	2	0	60	59	30,44	75		SW	1	Hazy.
27	45	7	0	45	58	30,48	83		E	1	Hazy.
	65	2	0	65	60	30,44	70		SW	1	Fine.
28	46	7	0	48	59	30,28	81		WSW	1	Fine.
	62	2	0	62	64	30,12	68		SW	1	Fine.
29	39	7	0	41	59	30,05	70		WSW	1	Fine.
	47	2	0	47	60	29,98	67		NW	2	Cloudy.
30	36	7	0	38	57	30,28	77		NE	1	Cloudy.
	49	2	0	49	60	30,33	63		NE	2	Fair.
31	32	7	0	34	57	30,48	76		NE	1	Hazy.
	48	2	0	48	58	30,43	66		NE	1	Hazy.

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1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom	Hygrometer.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.	°	Inches	Points.	Str	
April 1	36	7	0	38	56	30,31	75		E	1	Hazy.
	52	2	0	52	59	30,14	63		E	1	Fine.
2	35	7	0	36	56	29,85	78		E	1	Hazy.
	61	2	0	59	61	29,72	64		SSE	2	Hazy.
3	47	7	0	48	58	29,70	72		NE	1	Cloudy.
	58	2	0	58	60	29,74	70		ENE	1	Cloudy.
4	45	7	0	46	58	29,80	83		NE	1	Cloudy.
	61	2	0	61	61	29,81	67		NE	1	Hazy.
5	45	7	0	46	59	29,84	81		W	1	Fair.
	64	2	0	63	62	29,88	67		WSW	1	Fair.
6	44	7	0	46	60	30,22	69		NE	1	Fair.
	55	2	0	55	61	30,28	61		N	1	Fair.
7	40	7	0	42	59	30,33	76		SW	1	Fine.
	60	2	0	59	61	30,30	60		SW	1	Fine.
8	48	7	0	49	60	30,20	76		SW	1	Cloudy.
	66	2	0	66	61	30,11	66		SW	1	Cloudy.
9	48	7	0	49	61	30,00	80		N	1	Cloudy.
	56	2	0	55	61	29,98	73		NW	1	Rain.
10	43	7	0	45	60	30,01	78		W	1	Fair.
	58	2	0	58	61	30,01	63		NW	1	Cloudy.
11	50	7	0	50	59	29,70	81	0,030	WNW	2	Rain.
	55	2	0	55	60	29,78	58		WNW	2	Cloudy.
12	37	7	0	39	57	29,74	74	0,057	WNW	1	Fine.
	50	2	0	48	59	29,77	71		WNW	2	Cloudy.
13	37	7	0	40	56	29,96	76		NW	2	Fair.
	50	2	0	48	59	30,08	65		N	2	Cloudy.
14	33	7	0	35	55	30,26	73		W	1	Cloudy.
	54	2	0	54	58	30,21	63		W	1	Cloudy.
15	46	7	0	49	56	30,06	81	0,047	SW	1	Cloudy.
	58	2	0	57	58	30,06	69		SW	2	Cloudy.
16	43	7	0	48	56	29,94	82	0,043	SW	1	Rain.
	59	2	0	59	59	29,98	67		NW	1	Cloudy.

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1802	Six's Therm least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hy-gro-me-ter. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	42	7	0	46	57	30,18	78		W	1	Fair.
	64	2	0	63	59	30,19	66		SW	1	Cloudy.
18	51	7	0	51	58	30,27	83		SW	1	Cloudy.
	63	2	0	61	60	30,25	72		NW	1	Hazy.
19	51	7	0	53	59	30,30	84	0,093	E	1	Cloudy.
	67	2	0	66	61	30,30	74		S	1	Cloudy.
20	50	7	0	54	60	30,24	83		SW	1	Cloudy.
	68	2	0	67	62	30,20	65		NW	1	Fair.
21	49	7	0	49	60	30,22	77		NE	1	Cloudy.
	60	2	0	59	61	30,21	69		ESE	1	Fine.
22	47	7	0	51	60	30,02	78		SW	1	Cloudy.
	63	2	0	63	61	29,93	70		SW	2	Cloudy.
23	43	7	0	47	59	29,97	76		SW	1	Cloudy.
	57	2	0	53	60	30,05	75		W	1	Rain.
24	35	7	0	40	58	30,20	76	0,274	WSW	1	Fair.
	60	2	0	60	60	30,13	63		SW	1	Fair.
25	50	7	0	52	59	29,98	80		WSW	2	Cloudy.
	62	2	0	62	61	29,90	73		SW	2	Cloudy.
26	40	7	0	43	59	29,95	80	0,160	SW	1	Fine.
	60	2	0	60	61	29,86	62		SSW	1	Hazy.
27	43	7	0	49	58	29,63	78		SSW	2	Cloudy.
	55	2	0	54	59	29,57	80		SSW	2	Rain.
28	39	7	0	42	58	29,78	78	0,295	N	1	Cloudy.
	55	2	0	54	59	29,84	65		N	1	Cloudy.
29	42	7	0	43	57	30,00	78		NNE	1	Cloudy.
	52	2	0	52	58	30,03	70		NNE	1	Cloudy.
30	41	7	0	43	57	30,18	78		NNE	1	Cloudy.
	56	2	0	56	60	30,21	62		NE	1	Fair.

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1802	Six's Therm least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points	Str.	Weather.
	°						°				
May 1	40	7	0	46	56	30,18	68		NE	1	Cloudy.
	61	2	0	61	60	30,14	57		NE	1	Fine.
2	89	7	0	42	57	30,14	74		NE	1	Cloudy.
	63	2	0	61	60	30,07	60		NE	1	Fine.
3	40	7	0	44	58	30,04	68		NE	2	Hazy.
	60	2	0	58	59	30,14	67		NE	2	Cloudy.
4	39	7	0	43	57	30,27	70		NE	2	Fine.
	60	2	0	60	60	30,26	62		NE	2	Fine.
5	43	7	0	47	58	30,32	73		NE	1	Cloudy.
	56	2	0	56	59	30,33	68		NE	1	Cloudy.
6	45	7	0	47	58	30,32	72		NE	1	Cloudy.
	58	2	0	58	60	30,24	69		NE	1	Fair.
7	43	7	0	47	58	30,21	76		ENE	1	Cloudy.
	57	2	0	57	62	30,13	66		ENE	1	Fair.
8	40	7	0	45	58	29,97	82		NE	1	Cloudy.
	63	2	0	61	60	29,91	73		NE	1	Fair.
9	49	7	0	51	59	29,93	83		SW	1	Cloudy.
	63	2	0	63	60	29,96	70		SW	1	Fair.
10	47	7	0	50	60	30,07	70		NE	1	Fine.
	61	2	0	60	60	30,11	61		NE	1	Fine.
11	43	7	0	48	59	30,17	74		SW	1	Hazy.
	65	2	0	65	60	30,08	61		SW	1	Fine.
12	45	7	0	49	60	30,05	70		SW	1	Hazy.
	60	2	0	60	60	30,02	59		NW	1	Cloudy.
13	45	7	0	45	59	29,97	68		NW	1	Cloudy.
	53	2	0	53	59	29,97	59		N	1	Fair.
14	35	7	0	42	57	30,06	67		NE	1	Cloudy.
	50	2	0	50	57	30,04	60		NW	1	Cloudy.
15	32	7	0	38	56	30,05	67		NW	1	Hazy.
	51	2	0	50	55	29,98	65		WNW	1	Cloudy.
16	33	7	0	40	55	29,94	74	0,055	NE	1	Cloudy.
	48	2	0	48	55	29,94	67		NE	1	Hazy

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	°			°	°	Inches.	°	Inches			
May 17	33	7	0	37	54	29,97	74	0,022	W	1	Hazy.
	51	2	0	51	56	29,96	66		WNW	1	Hail.
18	31	7	0	35	54	30,02	73	0,047	W	1	Fair.
	55	2	0	55	56	29,99	62		WNW	1	Cloudy.
19	41	7	0	44	54	29,67	84	0,323	S	2	Rain.
	54	2	0	53	56	29,63	82		WNW	2	Cloudy.
20	38	7	0	43	54	30,01	78	0,094	ENE	1	Hazy.
	54	2	0	54	56	30,03	68		E	1	Cloudy.
21	41	7	0	47	55	30,05	83		NE	1	Hazy.
	73	2	0	73	59	30,06	56		ESE	1	Fine.
22	53	7	0	58	59	30,08	76		E	1	Fine.
	73	2	0	73	63	30,08	67		E	2	Fine.
23	51	7	0	55	60	30,06	69		ENE	1	Fine.
	65	2	0	65	62	30,06	60		E	2	Fine.
24	51	7	0	54	61	30,08	70		E	2	Fine.
	66	2	0	66	63	30,10	55		E	2	Fine.
25	48	7	0	56	61	30,01	70		NE	2	Fine.
	68	2	0	67	63	29,98	60		E	2	Fine.
26	57	7	0	60	63	29,93	68		E	2	Hazy.
	74	2	0	74	66	29,93	60		E	2	Fine.
27	54	7	0	57	63	29,99	70		E	2	Fine.
	71	2	0	70	66	29,99	55		E	1	Fine.
28	49	7	0	56	64	29,94	71		E	1	Fine.
	76	2	0	76	66	29,80	57		SE	2	Hazy.
29	60	7	0	62	66	29,56	74	0,052	SSW	1	Rain.
	69	2	0	68	66	29,51	68		ESE	1	Cloudy.
30	50	7	0	51	65	29,62	77	0,335	N	1	Cloudy.
	51	2	0	51	63	29,67	82		ENE	1	Rain.
31	41	7	0	43	62	29,84	81	0,268	NE	1	Cloudy.
	48	2	0	48	61	29,88	83		NE	1	Rain.

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1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M	o	o	Inches.		Inches.	Points.	Str.	
	o						o				
June 1	43	7	0	43	61	29,93	83	0,072	NE	1	Rain.
	54	2	0	54	61	30,02	71		NE	2	Rain.
2	40	7	0	46	60	30,13	75		NE	1	Fair.
	58	2	0	56	60	30,11	73		NE	1	Cloudy.
3	51	7	0	55	60	29,91	80		ESE	1	Cloudy.
	72	2	0	71	62	29,78	70		ESE	1	Cloudy.
4	55	7	0	59	61	29,67	90	0,285	S	1	Cloudy.
	69	2	0	69	62	29 67	65		SW	2	Cloudy.
5	58	7	0	60	62	29,61	83		SSW	1	Cloudy.
	70	2	0	68	63	29,58	68		SSW	2	Cloudy.
6	52	7	0	56	62	29 71	82		S	2	Cloudy.
	65	2	0	63	62	29,72	78		S	2	Rain.
7	54	7	0	56	62	29,75	80	0,051	SE	2	Cloudy.
	61	2	0	60	62	29,68	80		ENE	1	Rain.
8	52	7	0	56	62	29,58	84	0,618	W	1	Cloudy.
	66	2	0	66	63	29,66	65		WSW	1	Fair.
9	49	7	0	54	61	29,76	83	0,260	S	2	Rain.
	63	2	0	61	62	29 71	80		S	2	Rain.
10	56	7	0	58	61	29,64	86	0,165	S	2	Rain.
	62	2	0	62	62	29,65	82		S	2	Cloudy.
11	54	7	0	56	61	29,71	80	0,025	S	2	Fair.
	68	2	0	68	63	29,74	63		SSW	2	Fair.
12	51	7	0	56	61	29,85	78		SSW	2	Fair.
	67	2	0	67	63	29,85	67		S	2	Fair.
13	54	7	0	56	62	29,95	81	0,027	W	1	Fair.
	70	2	0	70	63	29,98	64		W	1	Fair.
14	50	7	0	54	62	30,10	75		WNW	1	Fair.
	69	2	0	68	63	30,13	63		NW	1	Fair.
15	52	7	0	56	62	30,16	75		SW	1	Cloudy.
	73	2	0	73	64	30,17	60		S	1	Fair.
16	58	7	0	60	63	30,17	80		NW	1	Hazy.
	78	2	0	77	64	30,17	64		W	1	Cloudy.

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1802	Six's Therm least and greatest Heat.	Time.		Therm. without.	Therm within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M	°	°	Inches.		Inches	Points.	Str.	
	°						°				
June 17	60	7	0	60	64	30,04	74		NW	1	Cloudy.
	67	2	0	67	64	30,05	67		NNE	1	Cloudy.
18	50	7	0	55	64	30,18	70		NE	1	Fine.
	70	2	0	70	64	30,18	60		NE	1	Fine.
19	50	7	0	55	64	30,15	73		SSW	1	Fine.
	75	2	0	74	64	30,09	62		WSW	1	Cloudy.
20	56	7	0	60	64	30,16	70		NW	1	Cloudy.
	73	2	0	73	64	30 26	63		NE	1	Fair.
21	49	7	0	54	64	30,38	75		E	1	Fine.
	66	2	0	66	65	30,36	63		E	1	Fine.
22	50	7	0	54	62	30,21	74		SW	1	Fine.
	67	2	0	67	66	30,08	61		W	1	Fine.
23	55	7	0	57	65	29,97	66		NW	1	Hazy.
	70	2	0	70	65	29,88	62		NW	2	Cloudy.
24	46	7	0	52	63	30,00	71		NW	1	Fine.
	65	2	0	65	64	30,05	61		NW	1	Cloudy.
25	46	7	0	52	63	30,05	70		NW	1	Cloudy.
	69	2	0	69	63	30,05	65		NW	1	Cloudy.
26	56	7	0	57	63	29,98	70		S	2	Fine.
	73	2	0	72	65	29,86	61		S	2	Fair.
27	52	7	0	57	63	29,67	76		SW	1	Cloudy.
	68	2	0	63	64	29,61	77		S	2	Rain.
28	56	7	0	58	64	29,55	82	0,032	SW	2	Cloudy.
	70	2	0	66	64	29,46	66		SW	2	Cloudy.
29	49	7	0	54	63	29,47	74	0,092	WSW	2	Fair.
	64	2	0	64	64	29,52	66		WSW	2	Fair.
30	44	7	0	50	62	29,76	80	0,235	W	1	Fine.
	67	2	0	67	63	29,72	63		W	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time. H. M	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°					°				
July 1	47	7 0	54	62	29,55	77		S	2	Rain.
	63	2 0	58	62	29,38	85		S	2	Rain.
2	51	7 0	55	62	29,51	77	0,133	SW	2	Fair.
	65	2 0	61	62	29,38	76		S	2	Rain.
3	49	7 0	53	61	29,58	72	0,135	W	2	Cloudy.
	64	2 0	62	61	29,66	75		W	2	Fair.
4	45	7 0	50	60	29,90	76	0,167	WSW	2	Fine.
	67	2 0	67	60	29,98	63		WNW	2	Fair.
5	49	7 0	53	60	30,08	75		W	2	Cloudy.
	66	2 0	64	61	30,08	68		SW	2	Cloudy.
6	57	7 0	58	60	29,96	83	0,038	SW	2	Cloudy.
	64	2 0	64	61	29,88	81		SSW	2	Cloudy.
7	54	7 0	58	60	29,93	78		WSW	1	Cloudy.
	71	2 0	70	62	30,00	64		WNW	1	Fair.
8	52	7 0	55	62	29,99	80		S	2	Fine.
	70	2 0	70	63	29,88	70		S	2	Cloudy.
9	53	7 0	55	62	29,79	82	0,280	WSW	1	Hazy.
	71	2 0	70	63	29,80	67		SW	2	Cloudy.
10	52	7 0	57	63	29,91	83	0,052	SSW	2	Rain.
	66	2 0	61	63	29.75	82		S	2	Rain.
11	53	7 0	55	62	29,87	73	0,065	W	1	Fair.
	65	2 0	65	63	29,93	63		WSW	2	Fair.
12	46	7 0	51	62	29,98	75		N	1	Hazy.
	65	2 0	64	63	29,98	61		N	1	Fair.
13	50	7 0	54	62	30,03	75		NE	1	Hazy.
	64	2 0	62	62	29,98	70		NE	1	Cloudy.
14	51	7 0	56	62	30,04	78	0,110	NNE	1	Cloudy.
	64	2 0	62	62	30,11	75		NE	1	Cloudy.
15	46	7 0	51	61	30,11	76		WSW	1	Fine.
	67	2 0	67	62	29,97	61		WNW	1	Fair.
16	49	7 0	53	61	29,90	78	0,100	NNE	1	Fair.
	67	2 0	65	62	29,91	75		WNW	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
July 17	53	7	0	56	61	29,74	83		SSW	1	Rain.
	68	2	0	68	63	29,65	68		WSW	2	Cloudy.
18	51	7	0	55	62	29,86	74	0,175	W	2	Fair.
	65	2	0	63	62	29,96	65		W	2	Fair.
19	51	7	0	53	61	29,87	82	0,260	ESE	1	Rain.
	66	2	0	64	62	29,70	81		S	1	Rain.
20	52	7	0	53	61	29,78	85	0,276	SW	1	Cloudy.
	68	2	0	68	63	29,77	64		SW	1	Cloudy.
21	53	7	0	54	62	29,59	80	0,083	E	1	Rain.
	65	2	0	64	62	29,49	78		S	2	Cloudy.
22	49	7	0	53	61	29,63	76	0,018	S	2	Fine.
	66	2	0	63	62	29,63	74		S	1	Rain.
23	53	7	0	55	61	29,76	83	0,217	WSW	1	Cloudy.
	66	2	0	66	62	29,90	70		W	1	Fair.
24	49	7	0	52	62	30,09	77		W	1	Fine.
	67	2	0	67	62	30,12	67		W	1	Cloudy.
25	51	7	0	53	62	30,09	76		W	2	Fair.
	70	2	0	68	62	30,03	65		SSW	2	Cloudy.
26	57	7	0	59	62	29,84	62		S	2	Cloudy.
	69	2	0	68	63	29,82	79		S	2	Cloudy.
27	57	7	0	58	62	29,80	81		SSW	1	Cloudy.
	69	2	0	67	63	29,82	78		SSW	1	Cloudy.
28	57	7	0	57	63	29,88	81		S	2	Cloudy.
	65	2	0	64	63	29,77	84		S	2	Rain.
29	55	7	0	57	63	29,79	81	0,102	S	1	Cloudy.
	70	2	0	70	64	29,77	70		S	1	Cloudy.
30	50	7	0	56	63	29,82	81	0,253	SW	2	Cloudy.
	70	2	0	68	64	29,81	68		SW	1	Rain.
31	49	7	0	53	63	29,86	83	0,352	SW	1	Fair.
	71	2	0	70	64	29,88	64		WSW	1	Fair.

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1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom	Hygrometer.	Rain.	Winds. Points	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Aug. 1	50	7	0	55	63	30,11	79		W	1	Fair.
	71	2	0	71	64	30,15	65		WNW	1	Fair.
2	57	7	0	60	64	30,18	80		WNW	1	Rain.
	67	2	0	67	64	30,17	76		SW	1	Cloudy.
3	60	7	0	62	64	30,14	90	0,075	WSW	1	Cloudy.
	76	2	0	76	66	30,14	72		WSW	1	Cloudy.
4	59	7	0	61	65	30,09	82		SSW	1	Fine.
	81	2	0	80	69	30,05	63		S	1	Fine.
5	59	7	0	63	67	30,05	84		E	1	Hazy.
	82	2	0	81	70	30,03	62		E	1	Fine.
6	66	7	0	68	69	29,93	72		SW	1	Fine.
	80	2	0	79	71	29,97	63		W	1	Fine.
7	59	7	0	61	67	29,95	61		SW	1	Fine.
	80	2	0	79	70	29,95	68		SW	1	Fair.
8	59	7	0	61	69	29,98	77		W	1	Cloudy.
	81	2	0	80	71	29,97	67		NE	1	Fine.
9	62	7	0	65	70	29,88	76		NE	1	Hazy.
	82	2	0	78	72	29,83	71		SW	1	Cloudy.
10	63	7	0	64	70	29,85	78		SSW	1	Fair.
	81	2	0	79	73	29,81	71		E	2	Cloudy.
11	60	7	0	62	70	29,86	75		W	2	Cloudy.
	78	2	0	77	71	29,94	64		SSW	2	Fair.
12	60	7	0	62	68	29,98	80	0,283	SSW	1	Rain.
	75	2	0	74	70	30,07	64		W	1	Fair.
13	52	7	0	55	69	30,27	74		NE	1	Fine.
	74	2	0	73	71	30,26	62		E	1	Fine.
14	53	7	0	58	69	30,28	76		NE	1	Hazy.
	74	2	0	73	70	30,28	64		NE	1	Fine.
15	54	7	0	58	69	30,28	75		E	1	Fine.
	79	2	0	79	70	30,20	61		S	1	Fine.
16	58	7	0	62	70	30,10	76		WSW	1	Hazy.
	78	2	0	77	70	30,10	68		NW	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches	Winds. Points.	Winds. Str.	Weather.
Aug. 17	64°	7	0	66	70	30,13	66°		NE	1	Cloudy.
	81	2	0	80	72	30,11	67		SSE	2	Fine.
18	59	7	0	65	70	29,96	72		NE	1	Fine.
	81	2	0	80	74	29,90	62		SSE	2	Fine.
19	64	7	0	64	70	29,87	76		S	1	Cloudy.
	78	2	0	78	74	29,87	65		S	2	Fair.
20	61	7	0	61	71	29,92	77		S	2	Fine.
	71	2	0	71	70	29,89	66		S	2	Cloudy.
21	60	7	0	60	70	29,89	77		SW	1	Fine.
	76	2	0	75	71	29,92	63		SSW	1	Fair.
22	61	7	0	61	69	29,97	78		S by E	1	Cloudy.
	77	2	0	75	69	29,96	69		S	2	Cloudy.
23	61	7	0	63	69	29,96	81		SSE	1	Cloudy.
	81	2	0	80	72	29,86	67		S	2	Fine.
24	62	7	0	63	70	29,86	78		SE	2	Cloudy.
	75	2	0	75	72	29,74	69		S	2	Cloudy.
25	54	7	0	56	69	29,77	72	0,016	SW	2	Fine.
	69	2	0	66	68	29,88	66		WSW	2	Fair.
26	53	7	0	55	68	30,05	75		WSW	2	Cloudy.
	64	2	0	62	67	30,09	73		NW	1	Rain.
27	52	7	0	53	66	30,19	79	0,115	NW	1	Rain.
	69	2	0	69	67	30,21	65		W	1	Cloudy.
28	62	7	0	63	67	30,12	90	0,028	W	1	Fair.
	78	2	0	78	69	30,26	71		W	1	Fair.
29	57	7	0	58	68	30,30	86		SSW	1	Foggy.
	79	2	0	79	71	30,27	69		S	1	Fine.
30	58	7	0	60	70	30,18	84		S	1	Foggy.
	82	2	0	81	73	30,14	66		S	1	Fine.
31	63	7	0	63	71	30,14	87		E	1	Cloudy.
	67	2	0	67	70	30,14	78		E	1	Cloudy.

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1802	Six's Therm least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom. Inches.	Hygiometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°			°	°		°				
Sept. 1	53	7	0	57	69	30,08	82		NE	1	Fine.
	69	2	0	69	71	30,06	66		E	1	Fine.
2	52	7	0	57	69	29,96	81		E	1	Fine.
	74	2	0	74	70	29,86	65		ESE	1	Cloudy.
3	61	7	0	64	69	29,74	85		SW	1	Fine.
	75	2	0	74	70	29,76	67		S	1	Cloudy.
4	64	7	0	64	69	29,58	85		S	2	Cloudy.
	75	2	0	75	71	29,58	68		S	2	Cloudy.
5	58	7	0	61	69	29,69	82	0,015	S	1	Fine.
	75	2	0	75	70	29,72	64		S	1	Fair.
6	58	7	0	59	69	29,83	83	0,320	E	1	Cloudy.
	70	2	0	70	69	29,82	73		S	2	Fair.
7	54	7	0	55	67	29,91	74	0,082	NW	1	Cloudy.
	63	2	0	63	68	29,98	62		NW	1	Fair.
8	43	7	0	46	65	30,13	73		NE	1	Fine,
	61	2	0	61	67	30,05	61		S	1	Cloudy.
9	46	7	0	52	65	29,79	73		E	1	Cloudy.
	71	2	0	71	67	29,74	65		SW	1	Fair.
10	60	7	0	62	66	29,50	78		SSW	2	Fair.
	65	2	0	62	66	29,42	81		SW	2	Rain.
11	48	7	0	50	64	29,76	74	0,255	WSW	1	Cloudy.
	62	2	0	62	64	29,81	64		WSW	1	Hazy.
12	40	7	0	42	62	29,94	77		W	1	Fine.
	60	2	0	60	64	30,02	65		W	2	Fair.
13	42	7	0	46	61	30,05	76		W	1	Fair.
	65	2	0	59	62	30,08	65		S	1	Cloudy.
14	52	7	0	55	61	30,10	85		S	1	Cloudy.
	67	2	0	67	63	30,15	69		SW	1	Cloudy.
15	57	7	0	58	62	30,22	83		WSW	1	Cloudy.
	75	2	0	73	64	30,24	66		W	1	Fair.
16	50	7	0	50	63	30,26	82		SW	1	Fine.
	73	2	0	73	65	30,24	67		S	1	Fine.

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1802	Six's Therm least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°			°	°		°				
Sep. 17	49	7	0	51	64	30,25	80		S	1	Fine.
	74	2	0	74	66	30,22	65		W	1	Fine.
18	51	7	0	53	65	30,15	79		NE	1	Fine.
	73	2	0	73	67	30,05	67		SE	1	Fine.
19	53	7	0	54	66	30,05	85		NE	1	Foggy.
	72	2	0	72	68	30,04	68		E	1	Fine.
20	55	7	0	55	67	30,06	88		NE	1	Foggy.
	71	2	0	71	69	30,02	69		E	1	Fine.
21	49	7	0	50	67	30,06	81		ENE	1	Fine.
	72	2	0	72	67	30,06	68		S	1	Fine.
22	55	7	0	56	67	30,14	84		SW	1	Cloudy.
	72	2	0	71	69	30,14	74		ESE	1	Fine.
23	51	7	0	52	68	30,16	81		NE	1	Fine.
	70	2	0	70	70	30,13	65		E	1	Fine.
24	51	7	0	52	67	30,15	83		NNE	1	Fine.
	68	2	0	68	69	30,18	62		E	1	Fine.
25	50	7	0	50	67	30,32	80		N	1	Fine.
	67	2	0	67	68	30,36	63		E	1	Fine.
26	47	7	0	48	66	30,41	79		NE	1	Cloudy.
	65	2	0	65	67	30,35	65		NNE	1	Fine.
27	55	7	0	55	66	30,30	77		NE	1	Cloudy.
	60	2	0	60	67	30,32	67		NE	1	Fair.
28	47	7	0	48	65	30,34	72		NE	1	Cloudy.
	58	2	0	58	65	30,34	68		NE	1	Fair.
29	54	7	0	55	65	30,38	76		WSW	1	Cloudy.
	67	2	0	67	66	30,37	70		NE	1	Fair.
30	55	7	0	55	65	30,35	83		E	1	Cloudy.
	65	2	0	65	66	30,30	76		S	1	Fair.

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1802	Six's Therm least and greatest Heat.	Time. H. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches	Winds. Points.	Str.	Weather.
Oct. 1	° 46	7 0	46	64	30,26	° 83		WSW	1	Cloudy.
	65	2 0	65	66	30,19	70		SW	1	Fine.
2	56	7 0	57	65	30,14	84		WSW	1	Fair.
	75	2 0	74	67	30,11	73		WSW	1	Fair.
3	54	7 0	54	65	30,01	85		SW	1	Fair.
	73	2 0	72	69	29,91	65		S	2	Fine.
4	58	7 0	61	66	29,65	76		S	2	Fair.
	63	2 0	63	66	29,75	74		S	2	Cloudy.
5	50	7 0	51	64	29,94	75	0,055	SSW	1	Fine.
	60	2 0	60	65	30,12	62		WNW	2	Fine.
6	45	7 0	46	63	30,09	73		SW	1	Fair.
	65	2 0	65	65	30,00	67		SW	1	Fair.
7	48	7 0	49	63	29,94	82		SW	1	Cloudy.
	65	2 0	65	64	29,99	66		SW	1	Fair.
8	52	7 0	52	63	29,73	82		SSW	1	Fine.
	65	2 0	65	64	29,65	77		S	2	Cloudy.
9	56	7 0	56	64	29,45	86	0,115	WNW	2	Rain.
	56	2 0	52	63	29,53	81		WNW	1	Rain.
10	40	7 0	40	61	29,86	80	0,260	WNW	2	Fine.
	53	2 0	53	62	30,02	66		WNW	2	Fine.
11	40	7 0	40	60	30,14	78		WNW	1	Cloudy.
	55	2 0	55	62	30,03	68		SSE	1	Fair.
12	50	7 0	53	60	29,65	85	0,208	S	2	Rain.
	63	2 0	61	60	29,54	89		SW	1	Rain.
13	47	7 0	48	59	29,92	77	0,150	NW	2	Cloudy.
	57	2 0	56	60	30,17	71		NW	2	Fair.
14	39	7 0	40	59	30,41	81		N	1	Cloudy.
	53	2 0	53	60	30,43	71		NNE	1	Fine.
15	38	7 0	38	59	30,40	79		NE	1	Fine.
	56	2 0	56	61	30,37	67		E	1	Fine.
16	39	7 0	39	59	30,20	82		NE	1	Fine.
	55	2 0	55	61	30,12	68		NE	1	Fine.

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1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm within.	Barom	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Oct. 17	33	7	0	33	58	29,92	79		NE	1	Foggy.
	49	2	0	49	58	29,79	77		SSE	1	Fine.
18	40	7	0	43	57	29,69	84		SW	1	Cloudy.
	58	2	0	55	58	29,68	84		SSW	1	Rain.
19	50	7	0	56	58	29,56	85	0,055	S	2	Cloudy.
	61	2	0	60	60	29,43	84		S	2	Rain.
20	47	7	0	47	59	29 54	86	0,335	SW	1	Fair.
	55	2	0	55	62	29,68	69		WSW	1	Fair.
21	43	7	0	47	58	29 89	82		SW	2	Fair.
	60	2	0	60	60	29,87	74		SSW	2	Fair.
22	56	7	0	56	59	29,83	81		S	2	Cloudy.
	59	2	0	59	60	29,78	77		S	2	Cloudy.
23	49	7	0	49	60	29,91	83	0,016	S	1	Cloudy.
	60	2	0	59	62	29,93	7[illegible]		S	1	Fair.
24	52	7	0	53	60	29,[illegible]9	86	0,225	SE	1	Rain.
	62	2	0	62	62	29,67	80		ESE	1	Fair.
25	56	7	0	56	61	29,40	85		ESE	2	Cloudy.
	57	2	0	57	60	29,40	53		S	1	Rain.
26	44	7	0	44	59	29 45	82	0,145	SE	1	Fair.
	56	2	0	55	60	29 50	77		S	2	Fair.
27	45	7	0	47	59	29,58	85		ENE	1	Cloudy.
	52	2	0	52	59	29,56	79		NE	1	Cloudy.
28	41	7	0	42	58	29,56	82	0,056	E	1	Fair.
	54	2	0	54	61	29,42	73		ESE	1	Fine.
29	47	7	0	51	58	29,22	83	0,021	E	2	Cloudy.
	55	2	0	55	59	29,27	77		NW	1	Cloudy.
30	37	7	0	38	58	29,35	81		NW	1	Cloudy.
	53	2	0	53	60	29,34	72		SSE	1	Fair.
31	37	7	0	37	57	29,44	81		NE	1	Fine.
	49	2	0	49	58	29,43	79		NE	1	Cloudy.

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1802	Six's Therm least and greatest Heat.	Time. H.	Time. M	Therm. without. °	Therm within. °	Barom. Inches.	Hygrometer.	Rain Inches.	Winds. Points.	Winds. Str.	Weather.
	°						°				
Nov. 1	34	7	0	34	56	29,49	84		W	1	Foggy.
	48	2	0	48	57	29,48	84		SW	1	Rain.
2	41	7	0	41	56	29,52	82		NNE	1	Cloudy.
	48	2	0	48	57	29,51	73		NE	1	Cloudy.
3	37	7	0	37	55	29,55	83		SW	1	Cloudy.
	45	2	0	45	55	29,61	83		WNW	1	Cloudy.
4	37	7	0	39	54	29,75	83		WNW	1	Fair.
	49	2	0	49	57	29,82	79		SW	1	Cloudy.
5	44	7	0	47	56	29,78	86	0,021	NE	1	Rain.
	49	2	0	49	57	29,74	88		ENE	1	Rain.
6	47	7	0	47	56	29,88	90	0,501	NNE	1	Cloudy.
	52	2	0	52	58	29,98	78		N	1	Cloudy.
7	34	7	0	34	55	30,00	85		NNE	1	Foggy.
	43	2	0	43	56	29,93	76		NE	1	Fair.
8	32	7	0	32	53	29,92	83		NE	1	Fine.
	44	2	0	44	56	29,95	76		NE	1	Fine.
9	32	7	0	32	53	30,13	82		NE	1	Fine.
	42	2	0	42	56	29,87	76		NE	1	Fine.
10	30	7	0	30	52	30,08	82		WSW	1	Fair.
	40	2	0	40	53	30,04	78		SSW	1	Fair.
11	33	7	0	38	52	29,69	83		E	1	Cloudy.
	41	2	0	41	53	29,58	78		ESE	1	Cloudy.
12	38	7	0	39	52	29,66	79		NNE	1	Fair.
	44	2	0	44	54	29,71	76		NNE	1	Cloudy.
13	38	7	0	40	52	29,78	79		NE	1	Cloudy.
	42	2	0	42	53	29,85	79		NE	1	Cloudy.
14	38	7	0	38	52	29,89	81		NE	1	Cloudy.
	40	2	0	39	53	29,86	79		NE	1	Cloudy.
15	38	7	0	38	51	29,72	84		NE	1	Cloudy.
	46	2	0	46	52	29,63	82		ENE	1	Cloudy.
16	40	7	0	40	52	29,64	85		NE	1	Cloudy.
	42	2	0	42	53	29,65	77		NE	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Nov. 17	41	7	0	41	52	29,78	80		NE	1	Cloudy.
	45	2	0	45	53	29,83	77		NE	1	Cloudy.
18	39	7	0	39	52	29 84	81		EN	1	Cloudy.
	43	2	0	43	54	29,76	81		ENE	1	Cloudy.
19	40	7	0	40	53	29,62	81		E	1	Cloudy.
	44	2	0	43	54	29,55	86		E	1	Rain.
20	43	7	0	43	53	29,48	90	0,155	S	1	Cloudy.
	46	2	0	46	55	29,40	89		ESE	1	Fair.
21	46	7	0	46	53	29,17	88		E	1	Cloudy.
	52	2	0	52	56	29,13	86		ESE	1	Fair
22	45	7	0	46	54	29,14	90	0,016	SE b E	1	Fair.
	53	2	0	53	56	29,17	86		SE	1	Fair.
23	48	7	0	48	56	28,63	90	0,062	E	1	Rain.
	51	2	0	51	57	28,80	90		W	1	Rain.
24	43	7	0	43	55	29,24	84	0,225	W	1	Fair.
	50	2	0	49	57	29,39	77		WNW	1	Fine.
25	43	7	0	45	56	29,57	85		SW	2	Cloudy.
	48	2	0	48	57	29,60	82		W	1	Cloudy.
26	39	7	0	42	56	29,40	82	0,034	E	1	Rain.
	47	2	0	47	57	29,37	87		E	1	Cloudy.
27	40	7	0	40	53	29,37	88		NE	1	Cloudy.
	45	2	0	45	56	29,36	85		NE	1	Cloudy.
28	36	7	0	36	55	29,49	88		NW	1	Cloudy.
	44	2	0	44	55	29,56	87		NE	1	Cloudy.
29	39	7	0	39	55	29,74	88		NE	1	Fair.
	44	2	0	44	56	29 79	83		NE	1	Cloudy.
30	38	7	0	38	53	29,98	86		NE	1	Fair.
	43	2	0	43	56	29,96	81		NE	1	Fair.

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for December, 1802.

1802	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches			
Dec. 1	35	8	0	37	53	29,74	87		NE	1	Cloudy.
	41	2	0	41	54	29,65	84		NE	1	Cloudy.
2	33	8	0	36	53	29,62	86		NE	1	Cloudy.
	46	2	0	46	55	29,62	75		E	1	Fair.
3	40	8	0	42	53	29,40	82		E	1	Cloudy.
	47	2	0	47	55	29,39	83		S	1	Cloudy.
4	35	8	0	35	52	29,52	85	0,110	S	1	Foggy.
	42	2	0	42	56	29,52	85		S	1	Fine.
5	35	8	0	35	52	29,46	87	0,022	NW	1	Cloudy.
	38	2	0	38	53	29,66	82		NW	1	Fair.
6	29	8	0	29	50	30,01	84		W	1	Fair.
	40	2	0	40	51	30,00	78		SW	1	Fine.
7	40	8	0	40	51	29,87	85		SW	1	Cloudy.
	41	2	0	41	53	29,77	78		SE	1	Fair.
8	30	8	0	30	50	29,71	83		E	1	Foggy.
	40	2	0	39	52	29,62	87		N	1	Rain.
9	39	8	0	39	51	29,60	93	0,130	NE	1	Foggy.
	42	2	0	42	52	29,62	93		NE	1	Cloudy.
10	33	8	0	40	51	29,48	89	0,025	SW	2	Cloudy.
	50	2	0	50	53	29,25	93		SW	2	Rain.
11	36	8	0	37	50	29,44	83	0,133	WNW	2	Cloudy.
	44	2	0	44	53	29,61	78		WNW	1	Cloudy.
12	33	8	0	33	50	29,97	82		WNW	1	Fair.
	40	2	0	40	53	30,00	78		SW	1	Fine.
13	39	8	0	43	51	29,78	93	0,045	W	1	Rain.
	45	2	0	44	53	29,81	79		NW	1	Fair.
14	33	8	0	34	51	30,00	86		W	1	Fine.
	41	2	0	41	53	29,98	83		WSW	1	Cloudy.
15	35	8	0	35	51	29,72	90		SW	1	Fair.
	48	2	0	44	53	29,66	89		SSW	1	Cloudy.
16	39	8	0	39	51	29,27	88	0,047	SSW	1	Fair.
	47	2	0	47	53	29,07	85		S	1	Cloudy.

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1802	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°						°				
Dec. 17	39	8	0	39	52	29,48	93	0,180	NE	2	Rain.
	42	2	0	42	54	29,71	77		NE	2	Cloudy.
18	32	8	0	32	51	30,27	78		NW	1	Fine.
	39	2	0	39	53	30,28	79		NW	1	Fine.
19	39	8	0	39	52	30,24	90		WSW	1	Cloudy.
	44	2	0	44	53	30,22	91		SW	1	Rain.
20	44	8	0	45	52	30,22	94	0,015			Foggy.
	48	2	0	48	54	30,22	94		SW	1	Cloudy.
21	46	8	0	46	53	30,18	94	0,026	WNW	1	Cloudy.
	45	2	0	47	56	30,19	77		NNE	1	Fair.
22	35	8	0	35	52	30,19	87		NNE	1	Cloudy.
	39	2	0	39	53	30,19	89		NNE	1	Cloudy.
23	36	8	0	38	52	30,19	90		NE	1	Cloudy.
	42	2	0	42	53	30,20	82		NE	1	Cloudy.
24	33	8	0	33	48	30,17	81		NE	1	Fair.
	39	2	0	39	53	30,07	73		NE	2	Fine.
25	33	8	0	33	51	29,92	79		ENE	1	Cloudy.
	34	2	0	34	52	29,90	79		ENE	1	Cloudy.
26	33	8	0	34	49	29,75	82		E	1	Cloudy.
	40	2	0	40	51	29,61	85		SE	2	Cloudy.
27	35	8	0	35	48	29,39	92	0,058	SW	1	Fair.
	45	2	0	45	51	29,34	91		S	1	Cloudy.
28	32	8	0	33	49	29,19	91	0,315	SW	1	Fair.
	38	2	0	38	52	29,28	80		WSW	1	Fine.
29	30	8	0	37	49	29,51	90		SSE	1	Cloudy.
	45	2	0	45	51	29,42	86		SSE	2	Cloudy.
30	40	8	0	44	50	29,24	91	0,047	S	2	Cloudy. [Much wind last night.
	46	2	0	46	53	29,37	89		S	1	Fine.
31	40	8	0	43	51	29,26	93	0,049	E	1	Cloudy.
	48	2	0	48	53	29,24	93		E	1	Cloudy.

1802.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain
	Greatest height.	Least height.	Mean height.	Greatest height	Least height	Mean height	Greatest height.	Least height.	Mean height.	Greatest height	Least height.	Mean height.	Greatest height.	Least height.	Mean height	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches	Deg.	Deg	Deg	Inches
January	48	15	34,6	48	16	35,4	54	41	47,4	30,46	29,31	29,93	90	64	80,2	0,146
February	56	30	40,8	55	30	41,1	57	48	52,6	30,16	29,19	29,67	90	72	80,5	1,500
March	62	27	43,1	62	29	43,7	64	51	55,2	30,48	29,32	30,08	90	60	74,9	0,397
April	68	33	51,0	67	35	51,8	62	55	59,1	30,33	29,57	30,22	84	60	72,6	0,989
May	76	31	52,9	76	35	54,0	66	54	59,5	30,33	29,51	30,08	84	55	68,7	1,196
June	75	40	59,6	77	43	61,0	66	60	62,8	30,38	29,46	29,90	90	60	72,3	1,862
July	71	45	59,1	70	50	60,0	64	60	62,0	30,12	29,38	29,86	85	61	74,7	2,816
August	82	50	67,9	81	53	68,2	74	63	69,1	30,30	29,74	30,04	90	61	72,4	0,517
September	75	40	58,3	75	42	60,9	71	61	66,3	30,41	29,42	30,05	85	61	73,6	0,672
October	75	33	50,9	74	33	52,8	69	57	61,1	30,43	29,22	29,80	89	62	77,8	1,641
November	53	30	42,3	53	30	42,7	58	51	54,5	30,13	28,63	29,63	90	73	82,7	1,014
December	50	29	39,3	50	29	39,5	56	48	52,0	30,28	29,07	29,73	94	73	85,5	1,196
Whole year			50,0			50,8			58,6			29,91			76,3	13,946

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIII.

PART II.

LONDON,

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MDCCCIII.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

XI. *Account of some Experiments on the Descent of the Sap in Trees. In a Letter from* Thomas Andrew Knight, *Esq. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read April 21, 1803.

MY DEAR SIR,

In a Memoir which I had the honour to present to you two years ago,* I related some experiments on trees, from which I inferred, that their sap, having been absorbed by the bark of the root, is carried up by the alburnum or white wood, of the root, the trunk, and the branches; that it passes through what are there called the central vessels, into the succulent part of the annual shoot, the leaf-stalk, and the leaf; and that it returns to the bark, through the returning vessels of the leaf-stalk. The principal object of this Paper is, to point out the causes of the descent of the sap through the bark, and the consequent formation of wood.

These causes appear to be, gravitation, motion communicated

* See Phil. Trans. for 1801, p. 333.

by winds or other agents, capillary attraction, and probably something in the conformation of the vessels themselves, which renders them better calculated to carry fluids in one direction than in another. I shall begin with a few observations on the leaf, from which all the descending fluids in the tree appear to be derived. This organ has much engaged the attention of naturalists, particularly of M. BONNET: but their experiments have chiefly been made on leaves severed from the tree; and, therefore, whatever conclusions have been drawn, stand on very questionable ground. The efforts which plants always make to turn the upper surfaces of their leaves to the light, have with reason induced naturalists to conclude, that each surface has a totally distinct office; and the following experiments tend strongly to support that conclusion.

I placed a small piece of plate glass under a large vine leaf, with its surface nearly parallel with that of the leaf; and, as soon as the glass had acquired the temperature of the house in which the vine grew, I brought the under surface of the leaf into contact with it, by means of a silk thread and a small wire, adapted to its form and size. Having retained the leaf in this position one minute, I removed it, and found the surface of the glass covered with a strong dew, which had evidently exhaled from the leaf. I again brought the leaf into contact with the glass, and, at the end of half an hour, found so much water discharged from the leaf, that it ran off the glass when held obliquely. I then inverted the position of the leaf, and placed its upper surface in contact with the glass: not the slightest portion of moisture now appeared, though the leaf was exposed to the full influence of the meridian sun. These experiments were repeated on many different leaves; and the result was, in

every instance, precisely the same. It seems, therefore, that in the vine, the perspiratory vessels are confined to the under surface of the leaf: and these, like the cutaneous lymphatics of the animal economy, are probably capable of absorbing moisture, when the plant is in a state to require it. The upper surface seems, from the position it always assumes, either formed to absorb light, or to operate by the influence of that body; and, if any thing exhale from it, it is probably vital air, or some other permanently elastic fluid. It nevertheless appears evident, in the experiments of BONNET, that this surface of the leaves of many plants, when detached from the tree, readily absorbs moisture.

Selecting two young shoots of the vine, growing perpendicularly against the back wall of my vinery, I bent them downwards, nearly in a perpendicular line, and introduced their succulent ends, as layers, into two pots, without wounding the stems, or depriving them of any portion of their leaves. In this position, these shoots, which were about four feet long, and sprang out of the principal stem about three feet from the ground, grew freely, and, in the course of the summer, reached the top of the house. As soon as their wood became sufficiently solid to allow me to perform the operation with safety, I made two circular incisions through the bark of the depending part of each shoot, at a small distance from each other, near the surface of the mould in the pots; and I wholly removed the bark between the incisions; thus cutting off all communication, through the bark, between the layers and the parent stems. Had the subjects of this experiment now retained their natural position, much new wood and bark would have been formed at the upper lip of the wounds, and none at all at the lower, as I

have ascertained by frequent experiment. The case was now different: much new bark and wood was generated on the lower lip of the wounds, become uppermost by the inverted position of the branches; and I have no doubt but that the new matter, thus deposited, owed its formation to a portion of sap, which descended by gravitation, from the leaves growing between the wounded parts and the principal stems.

The result of this experiment appears to point out one of the causes why perpendicular shoots grow with much greater vigour than others: they have probably a more perfect and more rapid circulation.

The effects of motion on the circulation of the sap, and the consequent formation of wood, I was best able to ascertain by the following expedient. Early in the spring of 1801, I selected a number of young seedling apple-trees, whose stems were about an inch in diameter, and whose height, between the roots and first branches, was between six and seven feet. These trees stood about eight feet from each other; and, of course, a free passage for the wind to act on each tree was afforded. By means of stakes and bandages of hay, not so tightly bound as to impede the progress of any fluid within the trees, I nearly deprived the roots and lower parts of the stems, of several trees, of all motion, to the height of three feet from the ground, leaving the upper parts of the stems and branches in their natural state. In the succeeding summer, much new wood accumulated, in the parts which were kept in motion by the wind; but the lower parts of the stems and roots increased very little in size. Removing the bandages from one of these trees in the following winter, I fixed a stake in the ground, about ten feet distant from the tree, on the east side of it; and I attached the

tree to the stake, at the height of six feet, by means of a slender pole about twelve feet long; thus leaving the tree at liberty to move towards the north and south, or, more properly, in the segment of a circle, of which the pole formed a radius; but in no other direction. Thus circumstanced, the diameter of the tree from north to south, in that part of its stem which was most exercised by the wind, exceeded that in the opposite direction, in the following autumn, in the proportion of thirteen to eleven.

These results appear to open an extensive and interesting field to our observation, where we shall find much to admire, in the means which nature employs to adapt the forms of its vegetable productions to every situation in which art or accident may deposit them. If a tree be placed in a high and exposed situation, where it is much kept in motion by winds, the new matter which it generates will be deposited chiefly in the roots and lower parts of the trunk; and the diameter of the latter will diminish rapidly in its ascent. The progress of the ascending sap will of course be impeded; and it will thence cause lateral branches to be produced, or will pass into those already existing. The forms of such branches will be similar to that of the trunk; and the growth of the insulated tree on the mountain will be, as we always find it, low and sturdy, and well calculated to resist the heavy gales to which its situation constantly exposes it.

Let another tree of the same kind be surrounded, whilst young, by others, and it will assume a very different form. It will now be deprived of a part of its motion, and another cause will operate: the leaves on the lateral branches will be partly deprived of light, and, as I have remarked in the last Paper I

had the honour to address to you, little alburnum will then be generated in those branches. Their vigour, of course, becomes impaired, and less sap is required to support their diminished growth: more, in consequence, remains for the leading shoots; these, therefore, exert themselves with increased energy; and the trees seem to vie with each other for superiority, as if endued with all the passions and propensities of animal life.

An insulated tree in a sheltered valley, will assume, from the foregoing causes, a form distinct from either of the preceding;* and its growth will be more or less aspiring, in proportion to the degree of protection it receives from winds, and its contiguity to elevated objects, by which its lower branches, during any part of the day, are shaded.

When a tree is wholly deprived of motion, by being trained to a wall, or when a large tree has been deprived of its branches, to be regrafted, it often becomes unhealthy, and not unfrequently perishes, apparently owing to the stagnation of the descending sap, under the rigid cincture of the lifeless external bark. I have, in the last two years, pared off this bark from some very old pear and apple-trees, which had been regrafted with cuttings from young seedling trees; and the effect pro-

* Not only the external form of the tree, but the internal character of the wood will be affected by the situation in which the tree grows; and hence, oak timber which grew in crowded forests, appears to have been mistaken, in old buildings, for Spanish chesnut. But I have found the internal organization of the oak and Spanish chesnut to be very essentially different. (See a magnified view of each in Plate IV.) The silver grain and general character of the oak and Spanish chesnut, are also so extremely dissimilar, that the two kinds of wood can only be mistaken for each other by very careless observers. Many pieces of wood found in the old buildings of London, and supposed to be Spanish chesnut, have been put into my hands; but they were all most certainly forest oak.

duced has been very extraordinary. More new wood has been generated in the old trunks, within the last two years, than in the preceding twenty years; and I attribute this to the facility of communication which has been restored between the leaves and the roots, through the inner bark. I have had frequent occasion to observe, that wherever the bark has been most reduced, the greatest quantity of wood has been deposited.

Other causes of the descent of the sap towards the root, I have supposed to be, capillary attraction, and something in the conformation of the vessels of the bark. The alburnum also appears, in my former experiments, to expand and contract very freely, under changes of temperature and of moisture; and the motion thus produced must be in some degree communicated to the bark, should the latter substance be in itself wholly inactive. I however consider gravitation as the most extensive and active cause of motion, in the descending fluids of trees; and I believe, that from this agent, vegetable bodies, like unorganized matter, generally derive, in a greater or less degree, the forms they assume; and probably it is necessary to the existence of trees that it should be so. For, if the sap passed and returned as freely in the horizontal and pendent, as in the perpendicular branch, the growth of each would be equally rapid, or nearly so: the horizontal branch would then soon extend too far from its point of suspension at the trunk of the tree, and thence must inevitably perish, by the compound ratio in which the powers of destruction, compared with those of preservation, would increase.

The principal office of the horizontal branch, in the greatest number of trees, is to nourish and support the blossoms, and the fruit or seed; and, as these give back little or nothing to the

parent tree, very feeble powers alone are wanted in the returning system. No power at all had been fatal; and powers sufficiently strong wholly to counteract the effects of gravitation, had probably been in a high degree destructive. And it appears to me by no means improbable, that the formation of blossoms may, in many instances, arise from the diminished action of the returning system in the horizontal or pendent branch.

I have long been disposed to believe the ascending fluids in the alburnum and central vessels, wherever found, to be everywhere the same; and that the leaf-stalk, the tendril of the vine, the fruit-stalk, and the succulent point of the annual shoot, might in some measure be substituted for each other; and experiment has proved my conjecture, in many instances, to be well founded. Leaves succeeded, and continued to perform their office, when grafted on the fruit-stalk, the tendril, and succulent shoot, of the vine; and the leaf-stalk, the tendril, and the fruit-stalk, alike supplied a branch grafted upon them with nourishment. But I did not succeed in grafting a fruit-stalk of the vine on the leaf-stalk, the tendril, or succulent shoot. My ill success, however, I here attribute solely to want of proper management; and I have little doubt of succeeding in future.

The young shoots of the vine, when grafted on the leaf-stalk, often grew to the length of nine or ten feet; and the leaf-stalk itself, to some distance below its juncture with the graft, was found, in the autumn, to contain a considerable portion of wood, in every respect similar to the alburnum in other parts of the tree.

The formation of alburnum in the leaf-stalk, seemed to point out to me the means of ascertaining the manner in which it is generated in other instances; and to that point my attention was in consequence attracted. Having grafted a great many

leaf-stalks with shoots of the vine, I examined, in transverse sections, the commencement and gradual formation of the wood. It appeared evidently to spring from the tubes which, in my last Paper, (to which I must refer you,) I have called the returning vessels of the leaf-stalk; and to be deposited on the external sides of what I have there named the central vessels, and on the medulla. The latter substance appeared wholly inactive; and I could not discover any thing like the processes supposed to extend from it, in all cases, into the wood.

The organization of the young shoot is extremely similar to that of the leaf-stalk, previous to the formation of wood within it. The same vessels extend through both; and therefore it appeared extremely probable, that the wood in each would be generated in the same manner: and subsequent observation soon removed all grounds of doubt.

It is well known that, in the operation of budding, the bark of trees being taken off, readily unites itself to another of the same or of a kindred species. An examination of the manner in which this union takes place, promised some further information. In the last summer, therefore, I inserted a great number of buds, which I subsequently examined, in every progressive stage of their union with the stock. A line of confused organization marks the place where the inserted bud first comes into contact with the wood of the stock; between which line and the bark of the inserted bud, new wood regularly organized is generated. This wood possesses all the characteristics of that from which the bud was taken, without any apparent mixture whatever with the character of the stock in which it is inserted. The substance which is called the medullary process, is clearly seen to spring

from the bark, and to terminate at the line of its first union with the stock.

An examination of the manner in which wounds in trees become covered, (for, properly speaking, they never can be said to heal,) affords further proof, were it wanted, that the medullary processes, (as they are improperly named,) like every other part of the wood, are generated by the bark.

Whenever the surface of the alburnum is exposed but for a few hours to the air, though no portion of it be destroyed, vegetation on that surface for ever ceases. But new bark is gradually protruded from the sides of the wound, and by this new wood is generated. In this wood, the medullary processes are distinctly seen to take their origin from the bark, and to terminate on the lifeless surface of the old wood within the wound. These facts incontestibly prove, that the medullary processes, which in my former Paper I call the silver grain, do not diverge from the medulla, but that they are formed in lines converging from the bark to the medulla, and that they have no connection whatever with the latter substance. And surely nothing but the fascinating love of a favourite system, could have induced any naturalist to believe the hardest, the most solid, and most durable part of the wood, to be composed of the soft, cellular, and perishable substance of the medulla.

In my last Paper, I have supposed that the sap acquired the power to generate wood in the leaf; and I have subsequently found no reason to retract that opinion. But the experiment in which wood was generated in the leaf-stalk, apparently by the sap descended from the bark of the graft, induces me to believe, that the descending fluid undergoes some further changes in

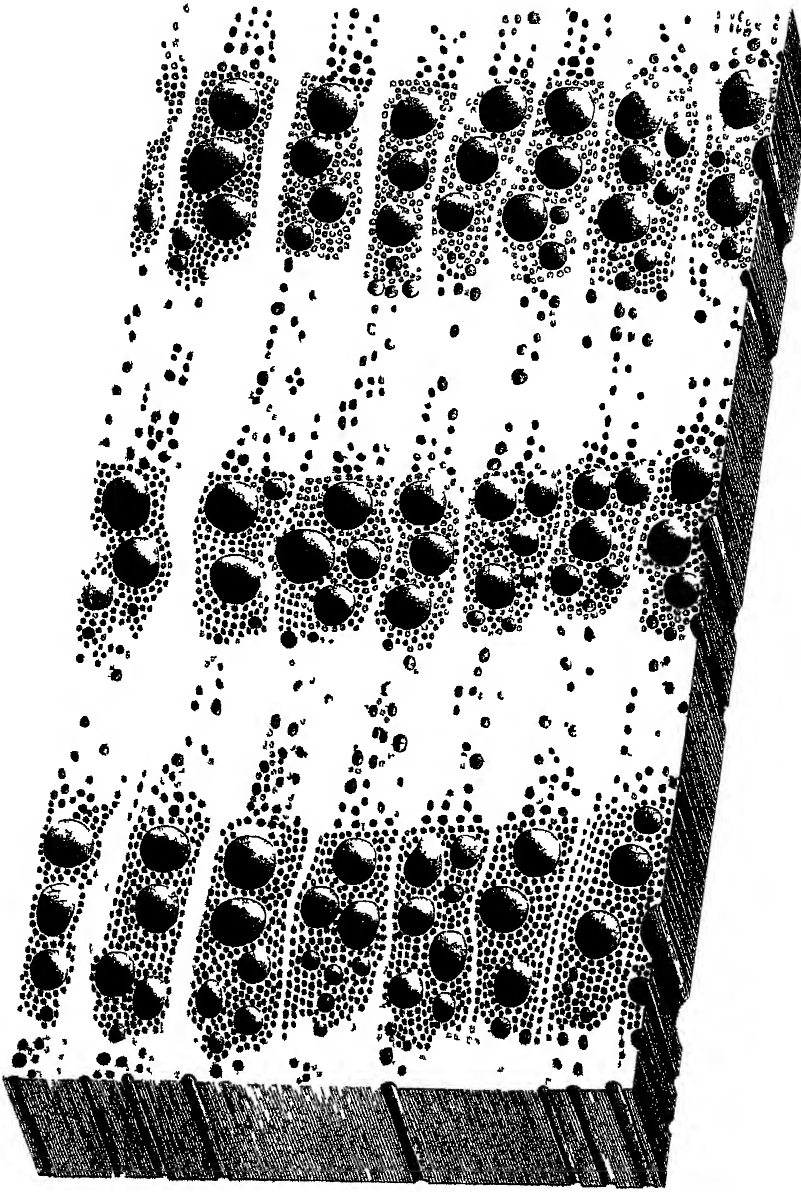
the bark, possibly by discharging some of its component parts through the pores described and figured by MALPIGHI.

I also suspected, since my former Paper was written, that the young bark, in common with the leaf, possessed a power, in proportion to the surface it exposes to the air and light, of preparing the sap to generate new wood; for I found that a very minute quantity of wood was deposited by the bark, where it had not any apparent connection with the leaves. Having made two incisions through the bark round annual shoots of the apple-tree, I entirely removed the bark between the incisions, and I repeated the same operation at a little distance below, leaving a small portion of bark unconnected with that above and beneath it. By this bark, a very minute quantity of wood, in many instances, appeared to be generated, at its lower extremity. The buds in the insulated bark were sometimes suffered to remain, and in other instances were taken away; but these, unless they vegetated, did not at all affect the result of the experiment. I could therefore account for the formation of wood, in this case, only by supposing the bark to possess in some degree, in common with the leaf, the power to produce the necessary changes in the descending sap; or that some matter originally derived from the leaves, was previously deposited in the bark: or that a portion of sap had passed the narrow space above, from which the bark had been removed, through the wood. Repeating the experiment, I left a much greater length of bark between the intersections; but no more wood than in the former instances was generated. I therefore concluded, that a small quantity of sap must have found its way through the wood, from the leaves above; and I found, that when the upper incisions were made at ten or twelve lines distance, instead of

one or two, and the bark between them, as in the former experiments, was removed, no wood was generated by the insulated bark.

I shall conclude my Paper with a few remarks on the formation of buds, in tuberous rooted plants, beneath the ground. They must, if my theory be well founded, be formed of matter which has descended from the leaves through the bark. I shall confine my observations to the potatoe. Having raised some plants of this kind in a situation well adapted to my purpose, I waited till the tubers were about half grown; and I then commenced my experiment by carefully intersecting, with a sharp knife, the runners which connect the tubers with the parent plant, and immersing each end of the runners, thus intersected, in a decoction of logwood. At the end of twenty-four hours, I examined the state of the experiment; and I found that the decoction had passed along the runners in each direction; but I could not discover that it had entered any of the vessels of the parent plant. This result I had anticipated; because I concluded, that the matter by which the growing tuber is fed, must descend from the leaves through the bark; and experience had long before taught me, that the bark would not absorb coloured infusions. I now endeavoured to trace the progress of the infusion in the opposite direction; and my success here much exceeded my hopes.

A section of the potatoe presents four distinct substances: the internal part, which, from the mode of its formation and subsequent office, I conceive to be allied to the alburnum of ligneous plants; the bark which surrounds this substance; the true skin of the plant; and the epidermis. Making transverse sections of the tubers which had been the subjects of the



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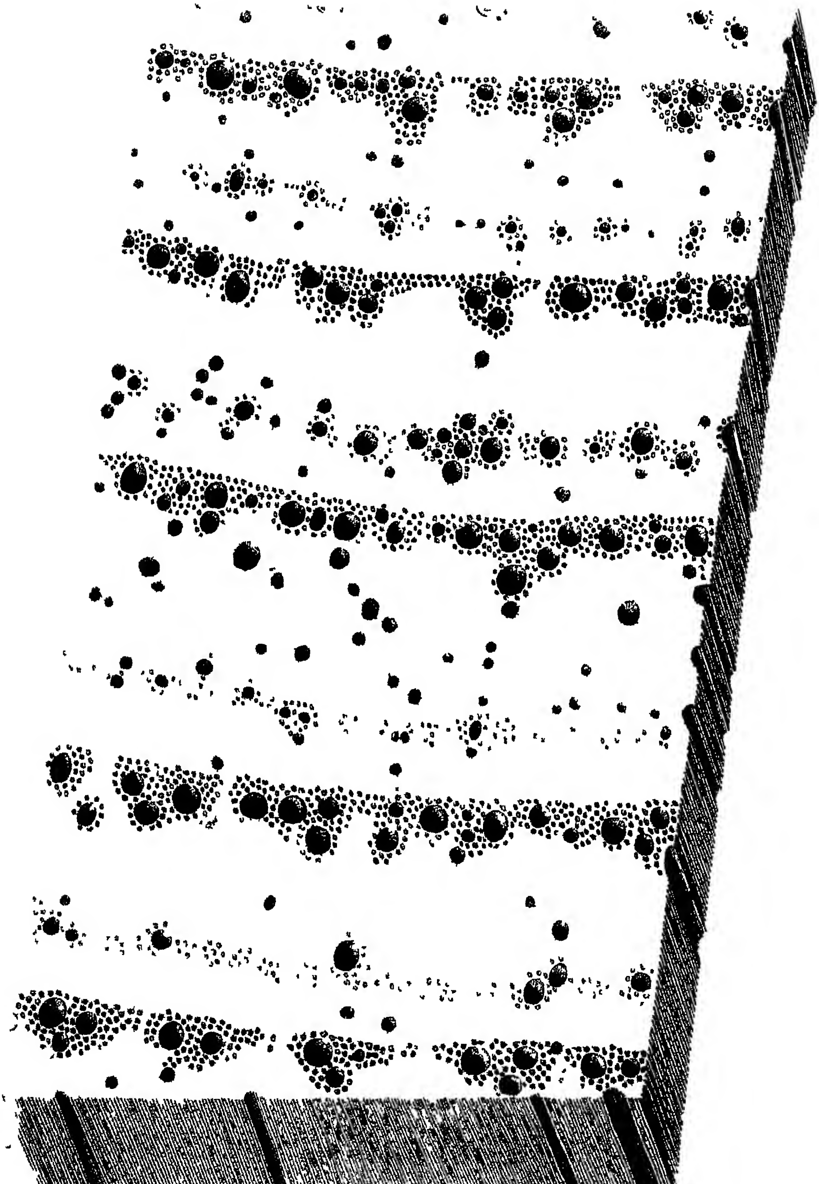


CHESNUT.

experiments, I found that the coloured infusion had passed through an elaborate series of vessels between the cortical and alburnous substances, and that many minute ramifications of these vessels approached the external skin at the base of the buds, to which, as to every other part of the growing tuber, I conclude they convey nourishment.

Some other experiments were made on this plant, which appeared to me interesting; but my Paper has already a good deal exceeded its intended limits. I will therefore dismiss the subject; but intend to trouble you with another Memoir in the autumn, should this be honoured with the approbation of the Royal Society.

I am, &c.

T. A. KNIGHT.

Elton,
March 26, 1803.

XII. *Enquiries concerning the Nature of a metallic Substance lately sold in London, as a new Metal, under the Title of Palladium. By* Richard Chenevix, *Esq. F. R. S. and M. R. I. A.*

Read May 12, 1803.

On the 29th of April I learned, by a printed notice* sent to Mr. Knox, that a substance, which was announced as a new metal, was to be sold at Mr. Forster's, in Gerrard-street. The mode adopted to make known a discovery of so much importance, without the name of any creditable person except the vender, appeared to me unusual in science, and was not calculated to inspire confidence. It was therefore with a view to detect what I conceived to be an imposition, that I procured a specimen, and undertook some experiments to learn its properties and nature.

* "Palladium, or new silver, has these properties amongst others that shew it to "be a new noble metal.

"1. It dissolves in pure spirit of nitre, and makes a dark-red solution. 2. Green "vitriol throws it down in the state of a regulus from this solution, as it always does "gold from aqua regia. 3. If you evaporate the solution, you get a red calx that dis-"solves in spirit of salt or other acids. 4. It is thrown down by quicksilver, and by "all the metals but gold, platina, and silver. 5. Its specific gravity by hammering, "was only 11.3; but by flatting, as much as 11.8. 6. In a common fire, the face of "it tarnishes a little, and turns blue, but comes bright again, like other noble metals, "on being stronger heated. 7. The greatest heat of a blacksmith's fire would hardly "melt it; 8. But, if you touch it, while hot, with a small bit of sulphur, it runs as "easily as zinc.

"It is sold only by Mr. Forster, at No. 26, Gerrard-street, Soho, London; in "samples of five shillings, half a guinea, and one guinea each."

' I had not proceeded very far, when I perceived that the effects produced by this substance, upon the various tests, were such as could not be referred, *in toto*, to any of the known metallic substances. I immediately returned to Mr. FORSTER, and became possessed of the whole quantity which had been left in his hands for sale. I could not obtain any information as to its natural state, or any trace that might lead to a probable conjecture.

The substance had been worked by art: it had been rolled out in flatting-mills; and was offered for sale in specimens consisting of thin laminæ. The largest of them were about three inches in length, and half an inch in breadth, weighing on the average 25 grs. and were sold for one guinea. The other laminæ were smaller, in proportion to the price.

Subjected to the same treatment as platina, to procure a polished surface, palladium assumed an appearance scarcely to be distinguished from that metal. The laminæ were not very elastic, but were very flexible, and could be bent several times in opposite directions without breaking. The specific gravity, I found to differ not a little from that which is stated in the printed notice, and to vary considerably in different specimens. Some pieces of the substance were as low as 10,972, while others gave 11,482.

The effects of GALVANIC electricity upon palladium, were the same as upon gold and silver. No oxidizement of the substance took place; but oxygen gas was emitted, during the whole time it formed a part of the GALVANIC circle in action.

A lamina of this substance being exposed to the blowpipe, the side removed from the immediate action of the flame became blue; but the temperature at which this colour was produced,

exceeded that at which steel begins to lose the tinge it had received at a lower heat.

I exposed palladium, in an open vessel, to a greater degree of heat than that which can melt gold. No oxidizement ensued; and, although the metallic slip was extremely thin, no appearance of fusion took place, even at the edges or corners. Upon increasing the fire considerably, I obtained a melted button; but I cannot estimate the degree at which the fusion was effected.

The button, by this treatment, had lost a little of its absolute weight; but its specific gravity had increased from 10,972 to 11,871. It was of a grayish-white. Its hardness was rather superior to that of wrought iron. By the file, it acquired the colour and brilliancy of platina. It was malleable to a great degree. Its fracture was fibrous, and in diverging striæ, which seemed to be composed of crystals; the surface of the button also, when seen through a lens, appeared to be crystallized.

Palladium very readily combines with sulphur. I exposed a certain quantity of it to a violent heat, without being able to melt it; and, at that elevated temperature, threw some sulphur upon it. It immediately entered into fusion, and remained in that state until the redness of the crucible was hardly visible in the daylight. The increase of weight in the button of the sulphuret, was such as could not indicate with exactness the proportion of sulphur combined with it; and I was so limited in the quantity of palladium I could obtain on any terms, that I thought it prudent to reserve as much as possible for the investigation of more important properties. Sulphuret of palladium is rather whiter than the substance itself, and is extremely brittle.

Palladium, melted in a charcoal crucible, and kept in fusion

for fifteen minutes, did not acquire any properties different from those which I have already mentioned, in speaking of the effect of heat upon that substance. Hence we may conclude, that there is not any action between charcoal and palladium.

I put equal parts of palladium and gold into a crucible, for the purpose of forming an alloy. The result, owing to an accident, did not weigh so much as the sum of the quantities employed; therefore, the proportions in this alloy were uncertain. Its colour was gray; its hardness about equal to that of wrought iron. It yielded to the hammer; but was less ductile than each metal separate, and broke by repeated percussions. Its fracture was coarse-grained, and bore marks of crystallization. Its specific gravity was 11,079.

Equal parts of platina and palladium, entered into fusion at a heat not much superior to that which was capable of fusing palladium alone. In colour and hardness, this alloy resembled the former; but it was rather less malleable. Its specific gravity, I found to be 15,141.

Palladium, alloyed with an equal weight of silver, gave a button of the same colour as the preceding alloys. This was harder than silver, but not so hard as wrought iron; and its polished surface was somewhat like platina, but whiter. Its specific gravity was 11, 290.

The alloy of equal parts of palladium and copper was a little more yellow than any of the preceding alloys, and broke more easily. It was harder than wrought iron; and, by the file, assumed rather a leaden colour. Specific gravity 10,392.

Lead increases the fusibility of palladium. An alloy of these metals, but in unknown proportions, was of a gray colour, and its fracture was fine-grained. It was superior to all the

former in hardness, but was extremely brittle. I found its specific gravity to be 12,000.

Equal parts of palladium and tin gave a grayish button, inferior in hardness to wrought iron, and extremely brittle. Its fracture was compact and fine-grained. Specific gravity 8,175.

With an equal weight of bismuth, palladium gave a button still more brittle, and nearly as hard as steel. Its colour was gray; but, when reduced to powder, it was much darker. Its specific gravity, I found to be 12,587.

Iron, when alloyed with palladium, tends much to diminish its specific gravity, and renders it brittle. Arsenic increases the fusibility of palladium, and renders it extremely brittle.

From these experiments, we may form the following Table, shewing the difference between the true and the calculated mean of specific gravity in the alloys of palladium.

	Metals.	Proportion.	Specific gravity by calculation.*	Specific gravity by experiment.	Difference.
Palladium alloyed with	Gold -	uncertain.	uncertain.	11,079	uncertain.
	Platina -	equal parts.	17,241	15,141	— 2,100
	Silver -	equal parts.	10,996	11,290	+ ,294
	Copper -	equal parts.	10,176	10,392	+ ,216
	Lead -	equal parts.	uncertain.	12,000	uncertain.
	Tin -	equal parts.	9,340	8,175	— 1,165
	Bismuth	equal parts.	10,652	12,587	+ 1,935

* In the specific gravities of the different metals, I have followed the Table given in

I exposed ten grains of palladium to the action of potash, in fusion, during half an hour. The substance lost its brilliancy, and diminished two grains and a half in weight: these were found in the potash.

The action of soda upon palladium, does not appear to be quite so violent.

Ammonia, allowed to remain for some days upon palladium, acquires a slight bluish tinge, and holds a small portion of oxide of palladium in solution. In all these cases, the action of the alkali is promoted by the contact of the atmospheric air, the oxygen of which combines with the metal, in favour of the affinity the oxide of palladium possesses towards the alkali.

Some of the pieces of palladium were more easily acted upon by the acids than others; and, in general, those of the greatest specific gravity were the least affected. Upon the whole, however, the following statement may be taken as the average of the habitudes of palladium with the acid solvents.

Sulphuric acid, boiled upon palladium, acquires a beautiful red colour, and dissolves a portion of the substance. The action of this acid is not very powerful, and, upon the whole, it cannot be looked upon as a good solvent for palladium.

Nitric acid acts with much greater violence upon palladium. It oxidizes the substance with somewhat more difficulty than it can oxidize silver; and, by dissolving the oxide, forms a very beautiful red solution. If the nitric acid be impregnated with nitrous gas, its action upon palladium is much more rapid.

Muriatic acid, by being boiled upon palladium for a considerable time, acts upon it, and becomes of a beautiful red.

But the true solvent of palladium is nitro-muriatic acid, which attacks it with great violence, and forms a beautiful red solution.

From all these acid solutions of palladium, a precipitate may be produced by the alkalis and earths. These precipitates are, for the most part, of a beautiful orange; are partly redissolved by some of the alkalis; and the supernatant liquor of the precipitate formed by ammonia is sometimes of a fine greenish-blue. Sulphate, nitrate, and muriate, of potash, or of ammonia, produce an orange precipitate in the salts of palladium, as in those of platina, when not in too dilute solution; and the precipitates from the nitrate of palladium are in general of a deeper orange. All the metals, except gold, platina, and silver, cause very copious precipitates in solutions of palladium. Recent muriate of tin produces a dark orange or brown precipitate in neutralised salts of palladium, and is an extremely delicate test. Green sulphate of iron precipitates palladium in the metallic state; and, if the experiment succeed, the precipitate is about equal in weight to the palladium employed. Prussiate of potash causes an olive-coloured precipitate; and water impregnated with sulphuretted hydrogen gas, a dark brown one. Fluoric, arsenic, phosphoric, oxalic, tartaric, citric, and some other acids, together with their salts, precipitate some of the solutions of palladium, and form various combinations with this substance.

Such are the principal characters I have found in palladium, examined as a simple metallic body. It does not appear that, in stating any of its properties, except its specific gravity, the printed notice has been guilty of misrepresentation.

From these experiments, it would be difficult to say of what metal, or of what combination of metals, palladium consists. We could not suppose gold or platina to be an ingredient in it, as it is in some measure acted upon by sulphuric and muriatic acids, and is wholly soluble in nitric acid. Silver is excluded, by

the effect of muriatic acid upon its solutions; as is lead, by that of the sulphuric. Tin, antimony, bismuth, or tellurium, would have left an insoluble residuum with nitric acid. No traces could be found of any of the acidifiable metals; and iron was looked for with particular care, but in vain. In a word, the precipitation by the metals, seems to exclude all those of easier oxidability than mercury; and this we should not suppose to be present, as copper is not in the least whitened, when used to precipitate palladium.

The striking similarity of many of the precipitates of palladium with those of platina, induced me to multiply the comparative experiments; and I constantly observed contradictory facts. The specific gravity, easy fusibility, combination with sulphur, precipitation by green sulphate of iron and by prussiate of potash, together with other effects, were such as I could not reconcile to the known characters of platina; unless I could suppose that a substance did exist, which could totally change its physical and chemical properties, or so disguise them as to render them proof against the evidence of chemical reagents.

The lightest of the metals is tellurium; yet, in order to produce an alloy of the specific gravity of palladium, (supposing for a moment the real density of the alloy equal to the calculated mean,) it would require two parts of tellurium and one of platina; and it is highly improbable, that so large a proportion of tellurium could exist in any mass, without being detected. We have been told of very extraordinary anomalies in chemical affinities, by Mr. Berthollet; and Mr. Hatchett has made us acquainted with some, not less extraordinary, in the properties of alloys. Yet I think we shall cease to wonder at what has been related by these chemists, when we learn that palladium is

not, as was shamefully announced, a new simple metal, but an alloy of platina; and that the substance which can thus mask the most characteristic properties of that metal, while it loses the greater number of its own, is mercury.

I confess it was not from an analysis of palladium that I was first led to this result; for I had convinced myself, by synthesis, of its nature, and had formed the substance, before I could devise any probable method of ascertaining its component parts.

In reflecting upon the various modifications which substances undergo when in union with each other, and on the variations produced in the laws of affinity by the intervention of new bodies, I was induced to try whether, by the affinity of platina with some metal easily reduced, it might not happen, that a reduction of both would take place by green sulphate of iron, although no such effect were produced upon each metal when separate. The most likely to succeed, as being most easily reduced, after gold, platina, and silver, was mercury. I poured some solution of green sulphate of iron into a salt of platina, and also into a salt of mercury; no precipitation took place. I united the two liquors; and a precipitate, exactly resembling that which is formed by green sulphate of iron in palladium, was instantly formed. I collected the precipitate, and exposed it to a strong heat; and, after repeated trials, obtained a metallic button, not to be distinguished from palladium.

It certainly is one of the most extraordinary facts respecting alloys, that two metals, by their union with each other, should so lose the characteristic properties of each individually, that neither of them can be immediately detected by the usual methods. Nothing but an affinity of the most powerful order could produce such effects. But, to place the metals under the

most favourable circumstances for that affinity to exert its influence, and to promote their union, is not the result of common methods. Among a great number which I have tried, many have failed, and none have been attended with uniform success. I have, however, formed palladium by the immediate union of platina and mercury; and, as whatever may place the apparent capriciousness of this combination in a conspicuous point of view is not devoid of interest, I shall describe the means by which I have attempted to produce it, whether they failed, or were attended with success.

SYNTHETICAL EXPERIMENTS.

Exper. 1. It was not till after repeated trials of the mode just mentioned, that I succeeded in forming palladium. In many instances, I obtained a button completely melted, of the specific gravity of 13, and sometimes more; not so easily fused by sulphur as palladium; not soluble in nitric acid; and the absolute weight of which exceeded that of the platina originally employed. But, although this substance was not platina, I could not say it was palladium. The most successful experiment by this method, was attended with the following circumstances. I dissolved one hundred grains of platina in nitro-muriatic acid, and then put in two hundred grains of red oxide of mercury, made by nitric acid; but this not being sufficient to saturate the excess of acid, I continued to add more, until it ceased to be dissolved. On the other hand, I prepared some green sulphate of iron, and poured it into a long-necked matrass. I then poured the mixed solution of platina and mercury into the solution of green sulphate of iron, and heated the whole upon a sand bath. In less than half an hour, a copious precipitate was formed; and the inside of the

matrass was lined with a thin metallic coat. The liquor was passed through a filtre, which I had weighed; and the precipitate, after digestion with muriatic acid, was well washed and dried. When I had collected as much of this as I could, there remained upon the filtre 12 grains; besides which, I had collected 264, in all 276. The supernatant liquor still contained a portion of mercury, and about eight grains of platina Therefore, the 276 were composed of 92 of platina, and 184 of mercury. From this it appears, that one hundred grains of platina, can determine the precipitation of near two hundred grains of mercury, by green sulphate of iron; and that, in this proportion, there is a reciprocity of saturation. The 264, collected from the filtre, were exposed to a low red heat, and were reduced to 144. The 12 of the filtre would have given about seven; therefore, the whole would have been 151. The substance was in the form of a fine powder, and had a metallic lustre. It was then put into a charcoal crucible, and fused into a button. This button weighed 128 grains, and, with the quantity left on the filtre, would have weighed 135. In this 135, there were 92 of platina; therefore, it was composed of about two parts of that metal and one of mercury. It was of the specific gravity of 11,2; was wholly soluble in nitric acid; was easily fused by sulphur; was precipitated by green sulphate of iron: in a word, it was not to be distinguished from palladium.

Exper. 2. As another mode of forming palladium in the humid way, I put metallic iron into a mixed solution of platina and mercury. Both metals were precipitated; and the precipitate was submitted to the same treatment as in the former case; but the success was not so complete. Iron can precipitate either platina or mercury separately; but green sulphate of iron can perform its function only in favour of the affinity of platina and

mercury. Their union is promoted by its action; and the effects are, in all probability, simultaneous. The combination of the metals takes place, if I may be allowed the expression, in their nascent metallic state, and in a fixed proportion of mutual saturation. The union of the two metals, therefore, is in the present experiment less intimate, and the button which results from fusing the precipitate, is of much greater density.

Exper. 3. The same process was repeated, only using zinc instead of iron, but the result was not more satisfactory.

Exper. 4. I poured some mercury into a solution of platina, and heated them together for some time. A precipitate took place; but, upon fusing it into a button, I did not find it to be palladium.

Exper. 5. I dissolved the same quantities of platina and mercury as in *Exper.* 1, in nitro-muriatic acid, and evaporated those solutions together. I then volatilized as much as I could of the mercury, at a red heat. At the end of the operation, I obtained precisely my original quantity of platina, reduced to the metallic state; but not one particle of the mercury remained along with it.

Exper. 6 and 7. The same quantities of platina and mercury, dissolved in nitro-muriatic acid, were precipitated by phosphate of ammonia; and the liquor was evaporated. The residuum, in a glassy state, was exposed to a violent heat in a charcoal crucible; and I obtained a melted button, which weighed more than the original quantity of platina, and was of the specific gravity of 14,5. On account of the easy fusibility of phosphuret of platina, I likewise tried to combine it directly with mercury, but could not succeed.

Exper. 8. I precipitated a mixed solution of platina and

mercury, by a current of sulphuretted hydrogen gas; and reduced the insoluble powder. After many attempts, in which I obtained buttons of the specific gravity of 14,3 and 14,5, I formed a piece weighing 11 grains, of the specific gravity of 11,5. This last was palladium; but I could not ascertain the excess of weight, as a part of the original precipitate had been lost.

Exper. 9. I mixed a solution of muriate of platina with prussiate of mercury, and obtained a slight precipitate. The liquor was evaporated, and the whole residuum exposed to a violent heat. This experiment did not succeed. It was not repeated so often as the others; but I have some reason to think it might be attended with success; for I obtained, in one instance a few very minute grains, that were soluble in nitric acid.

Exper. 10. I heated some purified platina, in the form of a very fine powder, with ten times its weight of mercury, and rubbed them together for a long time. The result was, an amalgam of platina. This amalgam, exposed to a violent heat, lost all the mercury it had contained; and the original weight of the platina remained without increase.

Exper. 11. The best method of forming an amalgam of platina, is that prescribed by Count MUSSIN PUSHKIN. I dissolved a known quantity of platina in nitro-muriatic acid, precipitated by ammonia, and evaporated the liquor. The residuum was rubbed for a long time with a great quantity of mercury and then exposed to a violent heat. Many operations failed; in some, I had a button of the specific gravity of 13,2. In one attempt, I succeeded completely: from 30 grs. of platina, treated as above, I obtained a button weighing 43,5, of the specific gravity of 11,736, which had all the properties of palladium.

Exper. 12. I fused together, in a charcoal crucible, 100 grs. of platina, 200 of cinnabar, 100 of lime, and 400 of calcined borax; and obtained a button, which weighed more than the platina, and was of the specific gravity of 15,7. It was not soluble in nitric acid; but combined with sulphur, at a red heat.

Exper. 13. In some experiments I had made, I found that the furnace in which I formed these alloys, was capable of melting platina, without the assistance of any flux except calcined borax. I therefore urged 100 grs. of platina, at a very strong heat; and, when I judged the fire to have attained its greatest intensity, I poured mercury upon the platina, through a long earthen tube that terminated in the crucible, and immediately withdrew the apparatus from the fire. No sensible union of the metals had taken place; nor had the platina increased in weight.

Exper. 14. I put 100 grains of platina into an earthen tube, and placed the tube horizontally in the above furnace. At one end of it was a retort, containing 2lbs. of mercury. When the tube was at its greatest heat, the mercury was made to boil; and the entire quantity passed over the surface of the platina, at that temperature. The experiment lasted one hour and a half; but the metals did not seem to have combined.

Exper. 15. Mr. Pepys was so obliging as to try the effect of his very powerful Galvanic battery, in forming palladium. A piece of platina-wire was plunged into a bason of mercury, and formed part of a Galvanic circle. The wire was nearly in fusion; but no combination seemed to take place. The nature of this experiment did not allow of very accurate weighing; but the fused globules of platina did not appear to have acquired the properties that constitute palladium.

Such are the experiments by which I attempted to form

palladium. They were chiefly founded upon two principles; disposing affinity, and assimilation. In the one case, I endeavoured to present to the metals that compose it, a substance which, on account of its affinity for some menstruum necessary for their solution, and of their own tendency to combine in the proportions stated in *Exper.* 1, might cause them to unite in the form of an insoluble compound. In the other case, I hoped to assimilate the properties of each, and, by making them something more alike, to place them in the most favourable circumstances for uniting. *Exper.* 1 was founded on the former, and *Exper.* 8 on the latter of these principles.

In many instances, when I did not form palladium, I obtained a metallic button which was not platina; and, when I did so, it always weighed more than the original quantity of platina employed. In repeating *Experiments* 1, 2, 4, 6, 8, 11, and 12, I seldom failed of having such a substance. No effect of this kind took place in any experiment, when mercury was not used along with platina; and the other metals were merely accessaries, in promoting their union and precipitation. This is sufficiently proved, by the uniformity of the results in different processes, whether it was palladium or the substance I now mention which was formed. The chief property which distinguishes the latter substance from platina, is its density. It is not unusual to obtain it of a specific gravity so low as 13; very frequently 15 or 17. In the first experiments, I suspected this lightness to be owing to some air-bubbles; but repeated fusion, and comparative experiments upon platina, soon convinced me of the contrary. The augmentation of weight also, which the platina never fails of acquiring, proves that this metal has combined with some ponderable substance; and, in fact, the result of these operations

is, an alloy which is a mean betwixt platina in its pure state and what has been called palladium. It is, consequently, subject to infinite variation. The first effects which mercury produces upon platina are, to render it more fusible, and to diminish its specific gravity. The next new property conferred upon it is, the power of uniting with sulphur; and, lastly, it becomes soluble in nitric acid. It is not however till the specific gravity is below 12, or 12,5 at most, that it has acquired this property; and all these effects follow the direct order of the increase of weight observable in the platina.

It is not very difficult to combine a small quantity of mercury with platina: but, to resolve the problem completely, and to produce an alloy of these metals which shall be of so low a specific gravity as 11,3, and shall be soluble in nitric acid, is not so easily accomplished. From the repeated failures which I have experienced in these operations, I am much inclined to think that the author of palladium has some method of forming it, less subject to error than any I have mentioned. No doubt that perseverance would put us in possession of his secret; but, being prevented by want of leisure from pursuing these researches at present, I have confined myself to establishing the fact, and describing the processes which I have employed.

Having thus acquired a certainty that mercury is a constituent part of palladium, I made some further experiments upon it, with a view to its analysis; but they have not been attended with so much success. It might be expected, from the great number of methods which have failed to form palladium, that many might be found to decompose it when formed. But I have found the converse of such processes as did not succeed in producing palladium, to be ineffectual in destroying the combination.

ANALYTICAL EXPERIMENTS.

Exper. 1, 2, and 3. The converse of the synthetical experiments 1, 2, 3, was made, but without any satisfactory result.

Exper. 4. The converse of *Exper.* 4 was made without success. I put some mercury into a solution of palladium, and left them together for some time. The precipitate which was formed was palladium, just as it had been used for the operation.

Exper. 5. I exposed different pieces of palladium to a very violent heat for two hours. In some, a diminution of absolute weight, with an increase of specific gravity, took place; in others, neither of these effects was produced. The specimens which I had made were chiefly of the latter kind.

Exper. 6. Cupellation did not afford any satisfaction respecting the analysis of palladium; but the heat necessary for this purpose is so great, that I could not place great reliance upon this experiment. It is difficult to detach the button from the cupel with accuracy.

Exper. 7. I burned some palladium in oxygen gas. A white smoke arose during the combustion, and was deposited upon the sides of the glass jar that contained the gas. But this smoke was palladium, and not the mercury separated from it.

Exper. 8. A slip of palladium, which Mr. DAVY had the goodness to expose, in my presence, to the action of the strong GALVANIC batteries of the Royal Institution, burned with a very vivid light, and a white smoke; but no mercury was separated by this operation.

There is not any property of this compound which appears to me so wonderful, as that which is manifested by these experiments. It is a striking proof how unfounded was the opinion

of some philosophers, who supposed that the rapidity of combination was a measure of the force of affinity. We do not know of any affinity among chemical bodies which is more powerful than that of platina and mercury appears to be. The obstacles which must be overcome, in order to fix the latter metal, are a proof of this; yet the difficulty of forming this combination to its full extent is extreme. The difference which exists between the compound and its elements, when merely mixed, either in solution or otherwise, cannot be better exemplified than by comparing the result of the 5th synthetical experiment, with the difficulty of expelling mercury from the compound.

I must here observe, that all the analytical experiments, and many others, were made, by way of comparison, upon the palladium I had bought, as well as upon that which I had made. But, although I had myself combined the mercury with the platina, and consequently knew it to be in the compound that resulted, I could not succeed in separating it. Neither did the substance described in a former paragraph, as intermediate between platina and palladium, allow one particle of mercury to escape from it, by any process I have yet been able to devise.

The name of palladium conveys to our mind the idea of something absolute, and therefore incapable of gradation. But gradations in alloys are infinite; and the alloy of platina and mercury is susceptible of infinite variation. Palladium also brings to our recollection a contemptible fraud directed against science: the name, therefore, should not be admitted. I have called it an alloy; for it differs too much from the usual idea we have annexed to the word amalgam, but it accurately corresponds with our notions of the name I have adopted.

The facts which I have related in this Paper, appear at first

sight to have no similar examples in chemistry; and may not gain immediate assent from every person. The philosopher, indeed, will feel no humiliation in being forced to correct or to extend his knowledge; and will not altogether disbelieve a fact, because he can adduce no parallel instance, or because it is not in unison with his received opinions. Such conduct would be raising an insurmountable barrier against the progress of science: it would be setting up our own feelings in the place of nature; and attempting to measure what in itself is immeasurable, by the narrow scale of human comprehension.

But let us not confine our view of the facts and principles that have been mentioned, to this single instance. Let us trace them in a more extended circle; and see whether any thing may be found in nature that can apply to the present subject.

The first prejudice, for such I must call it, against the presence of platina in palladium is, the small density of the alloy. And no doubt it is extraordinary, that a metal the specific gravity of which is at least 22, (CHABANEAU says 24,) combined with another the specific gravity of which is nearly 14, should produce a mass of the specific gravity of 10,972; not much more than half of that which calculation would denote, and inferior to either of its elements. In Mr. HATCHETT's Paper upon the Alloys of Gold, to which I always refer with pleasure, we find some extraordinary instances of anomalies in specific gravity, both in excess and diminution upon the calculated mean. His experiments have not been doubted; nor can their accuracy be called in question. The principle of deviation in the true and the calculated mean is therefore admitted. Who then can say where this deviation shall end, or mark out limits to the operations of nature?

But a no less extraordinary instance of irregular density is daily before our eyes; yet it has not so much as attracted our attention. It is true that it is taken from among the gases. But, if we suppose that we have attained accuracy in experiments upon these subjects, I see no reason to refuse their evidence in this instance. The density of oxygen gas, to that of water, is as 1 to 740; and the density of hydrogen gas as 1 to 9792. The mean density of that proportion of oxygen and hydrogen gases which constitutes water, is to that of water as 1 to 2098; or, in other words, water is 2098 times heavier than the mean density of its elements in the gaseous state. But water is only 1200 times heavier than steam, or water in the state of vapour. Therefore, there is a variation in +, of 898, or nearly half, between the density of water and its elements, when both are in the aeriform state. This fact, however, regards bodies only as they remain in the same state, whether of solidity, liquidity, or fluidity. The anomaly is much greater, if we contemplate them as they pass from one of these states to the other. Yet we must not omit the consideration of such a change, in the instance of mercury alloyed with platina; for the former metal, before liquid, becomes solid as it enters into the new combination.

A stronger prejudice will perhaps exist against the fixation of so volatile a substance as mercury. It is certain that the labours of the alchemists have thrown some ridicule upon this subject, as a philosophical pursuit. Men of science have long since declined the research; and it is not probable that we are indebted to experiments undertaken in the true spirit of philosophy, for the present fixation of mercury. But, the same cause which induced us to look upon the project as chimerical, should dispose us to admit it when accomplished. Every chemist well knows,

that similar fixations of volatile substances are not uncommon. If an ore containing sulphur, or arsenic, or antimony, be gently roasted, a great part of those volatile bodies is driven off; but, if a fusing heat be suddenly applied, the mass unites in such a manner that a very small share of them escapes. Mr. HATCHETT has instanced an artificial combination of gold and arsenic, from which he could not expel the latter metal, by any degree of heat. Yet arsenic, though less fusible, is not much less volatile than mercury. I will also add a case still more in point; *viz.* the combination of arsenic and platina, which is not to be broken by a fusing heat.

An example of this fact, occurs again in water. The liquefaction or solidification of two gases to produce water, by a loss of caloric, never shocks our mind, because it is familiar to us. We cannot say what loss of caloric may be sustained by mercury, in order to unite with platina; or how far the presence of the latter may contribute to expel caloric from the former. We know too, that at any temperature, without the aid of a combustible body, to act as a reductive, we have not been able to disunite the last portions of oxygen, from the oxides of iron or of manganese. Yet, in the usual method of reducing a metallic oxide, the oxygen is surrounded by a much greater quantity of caloric than is necessary to convert it into gas. Every fixation of a volatile substance is analogous to the present question; and they whose minds have taken alarm from the novelty of the fact, may thus be familiarized with the necessity of admitting it.

But, it may be objected, in the instances of iron or manganese, oxygen is combined with a combustible body, and retained in it by a decided and powerful affinity. There is no reason to suppose

that such an affinity may not exist among metals. We have been forced to acknowledge it, in a few cases, among the earths; and, from the profound and sagacious researches of Mr. BERTHOLLET, we have learned many new facts, that promise us a rapid increase of knowledge. I shall beg leave to add a few examples, which are taken from that class of bodies to which the subject of the present Paper belongs, and show that the metals obey the general law of mutual attraction.

EXPERIMENTS TO PROVE AFFINITY AMONG THE METALS.

Exper. 1. I dissolved one hundred grains of silver in nitric acid, and precipitated by neutral muriate of platina. The precipitate, well washed and dried, was of a bright straw-colour, and weighed 147 grs. Reduced in a charcoal crucible, it yielded a button weighing 121 grs. and of the specific gravity of 11,6. The difference of weight, between the original hundred grains of silver and the 121, was owing to 21 grains of platina, which had been drawn down in precipitation along with the silver, by an affinity for that metal. This alloy is acted upon by nitric acid, and a great part of the platina is dissolved along with the silver; nor is it very easy to separate them by the common methods.

Exper. 2. I dissolved one hundred grains of silver in nitric acid, and added about 1200 of mercury. I poured the mixed solution into a solution of green sulphate of iron, and obtained a very copious precipitate. When washed and dried, it weighed 939, and was a perfect amalgam, in the due proportion of mutual saturation. Its specific gravity was 13,2; but no mercury remained with it after exposure to heat.

Exper. 3. I dissolved one hundred grains of gold in nitro-muriatic acid, and added to it about 1200 grains of mercury.

Green sulphate of iron, poured into this mixed solution, caused a precipitate weighing 874. It was in the form of a fine blue powder, not resembling an amalgam, though wholly metallic. Its specific gravity I could not ascertain; but all the mercury was expelled by heat.

The reagents which I used in the following experiments, were recent muriate of tin, and green sulphate of iron. To bring the examples of anomalous precipitations, in mixed solutions of the metals, more clearly into view, it will be necessary to state the action of these salts, upon a solution of each metal when separate.

By recent muriate of tin we have, with a solution of gold, the well known purple of CASSIUS. With platina, the colour of the liquor is much heightened. With mercury, there is a total reduction. With copper, a reduction from the black oxide at 20 *per cent.* of oxygen, to the yellow oxide at 11,5 *per cent.* of oxygen. With arsenic acid, a reduction to the state of white oxide. With silver, with lead, with antimony, no reduction. Green sulphate of iron reduces none of the metallic solutions, except those of gold and of silver.

When mixed solutions of the metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, we have the following results.

Experiments 4, 5, 6, 7, and 8. Muriate of tin, poured into a mixed solution of gold and mercury, precipitates both metals together; and there are no traces of the purple. Mixed solutions of gold and antimony, also of gold and arsenic acid, are acted upon in the same manner. Mixed solutions of gold and copper, also of gold and lead, afford results similar to those of each metal when separate.

Experiments 9, 10, 11, 12, and 13. With a solution of platina and arsenic acid, muriate of tin gives no precipitate; but the colour of the liquor is more heightened than if the platina had been alone in solution. Platina and antimony give a precipitate by this reagent, after standing some time; but the effect is retarded by the excess of acid in the solution of antimony. Platina and copper, also platina and lead, are acted upon as the separate solutions of these metals. Platina and silver are precipitated together by green sulphate of iron.

Experiments 14, 15, 16. Mercury and copper, mercury and lead, also mercury and arsenic, are precipitated in the metallic state by recent muriate of tin.

From these experiments it is evident,

1st. That gold has an affinity for mercury, for antimony, and for arsenic.

2d. That platina has an affinity for silver, for mercury, and for antimony; and that it is influenced by the presence of arsenic.

3d. That silver has an affinity for mercury.

4th. That mercury has an affinity for copper, for lead, and for arsenic.

This series of experiments is not intended as a system of metallic affinities; but as a few facts stated to corroborate an assertion. I am well aware that many others might be noticed; but it is not my intention to enter further into this subject, in the present Paper. The general importance of the principle, and the extensive influence it is likely to have upon chemistry, demand that it should be treated by multiplied researches. The experiments that can elucidate it are of the most delicate nature, and require peculiar care; for they do not always succeed, unless performed under the most favourable circumstances.

When mixed solutions of three or more metals are exposed to the action of recent muriate of tin, or of green sulphate of iron, their action upon each other appears in a much more striking, as also in a much more complicated point of view.

EXPERIMENTS UPON PLATINA.

I shall now state some experiments which I have had occasion to make upon platina, during the foregoing researches. Very little is known concerning this metal, its oxides, or its salts; and, although I have not had occasion to extend the enquiries very far, yet my experiments may serve to establish a few points.

I dissolved a quantity of purified platina* in nitro-muriatic acid, and precipitated by lime. A great portion of platina remained in the liquor, although I had used an excess of the above earth. I redissolved the precipitate in nitric acid, and evaporated to dryness. The result was, a subnitrate of platina. I then exposed the mass, in a crucible, to a heat capable of expelling the acid altogether; and the oxide remained alone. When this was reddened, at a heat which certainly was not capable of melting silver, the oxide was reduced, and appeared with a metallic lustre. The weight of the various products, in the above experiments, was such as to give the following proportions in the oxide, and the subnitrate of platina.

Yellow oxide of platina is composed of,

Platina - - - -	87
Oxygen - - - -	13
	100.

* By purified platina, I have always understood, in this Paper, platina reduced, at a gentle heat, from the salt obtained by pouring a concentrate solution of muriate of ammonia into a concentrate solution of platina.

Subnitrate of platina is composed of,

The above oxide of platina - -	89
Nitric acid and water - - -	11
	100.

But, in the reduction of this oxide of platina, it became of a green colour; and remained during some time in that state. Nitrate of platina sometimes becomes of a pale green at the edges, when evaporated to dryness; and ammonia assumes a green colour when it holds oxide of platina in solution, as we have seen more particularly with palladium. This, therefore, is a second oxide of platina. It contains but seven *per cent.* of oxygen.

I dissolved a known portion of platina in nitro-muriatic acid, and expelled the nitric acid, by pouring in a sufficient quantity of the muriatic; and then evaporated to dryness. By this experiment I learned, that the insoluble muriate of platina is composed of,

Yellow oxide of platina - -	70
Muriatic acid and water - -	30
	100.

I then expelled the muriatic acid by the sulphuric, and evaporated again to dryness. I found the insoluble sulphate of platina to be composed of,

Oxide of platina - - -	54,5
Acid and water - - - -	45,5
	100,0.

By much the most delicate test for platina is muriate of tin. A solution of the former, so pale as hardly to be distinguished from water, assumes a bright red by a single drop of the recent

muriatic solution of the latter metal. If mercury be present, the colour is much darker. Recent muriate of tin, poured into a solution of the muriate formed by the red oxide of mercury, converts it into the muriate formed by the less oxygenized acids; but, shortly after, the mercury is reduced to the metallic state. Hence it was, that the alloy of platina and mercury always gave a deeper coloured precipitate than platina, with muriate of tin.

Neither platina nor mercury are precipitated by prussic acid, or by the prussiates. But, if sulphate, nitrate, or muriate of platina be poured into prussiate of mercury, an orange-coloured precipitate is immediately formed; and, in some cases, a mixed solution of platina and mercury gives a similar precipitate by prussic acid alone.

Platina is one of the metals which are precipitated by sulphuretted hydrogen, without the necessity of a double affinity.

The affinities of platina differ much from what is generally stated in the tables. By the few acids I have had occasion to try, oxide of platina is attracted in the following order: sulphuric, oxalic, muriatic, phosphoric, fluoric, arsenic, tartaric, citric, benzoic, nitric, acetic, and boracic.

That sulphuric acid should attract the oxide of platina with greater force than the muriatic, is an unanswerable argument to an opinion which was long supported by many philosophers, and which is not yet altogether abandoned by them. Muriatic acid has been said to contribute to the solution of gold or platina, in nitro-muriatic acid, in the same manner as sulphuric acid is supposed to promote the decomposition of water, during the solution of iron by that acid diluted. The affinity of muriatic acid for the oxide of gold or of platina, has been looked upon

as the disposing cause that nitric acid is decomposed by those metals. But it is evident that some other action takes place; for, sulphuric acid, which has a stronger affinity for oxide of platina than muriatic acid, does not in the least promote the decomposition of nitric acid by gold, or by platina.

CONCLUSION.

The substance which has been treated of in this Paper, must convince us how dangerous it is to form a theory before we are provided with a sufficient number of facts, or to substitute the results of a few observations, for the general laws of nature. If a theory is sometimes useful, as a standard to which we may refer our knowledge, it is at other times prejudicial, by creating an attachment in our minds to preconceived ideas, which have been admitted without inquiring whether from truth or from convenience. We easily correct our judgment as to facts; and the evidence of experiment is equally convincing to all persons. But theories, not admitting of mathematical demonstration, and being but the interpretation of a series of facts, are the creatures of opinion, and are governed by the various impressions made upon every individual. Nature laughs at our speculations; and though from time to time we receive such warnings as should awaken us to a due sense of our limited knowledge, we are presented with an ample compensation, in the extension of our views, and a nearer approach to immutable truth.

The affinities of metals for each other are likely to be of the most extensive influence in chemistry. They will promote scepticism with regard to future discoveries, and throw some doubts upon our present knowledge. Palladium is certainly not less different from the elements that compose it, and from all other

metals, than any two can be from each other. Within the last fifteen or twenty years, several new metals and new earths have been made known to the world. The names that support these discoveries are respectable, and the experiments decisive. If we do not give our assent to them, no single proposition in chemistry can for a moment stand. But, whether all these are really simple substances, or compounds not yet resolved into their elements, is what the authors themselves cannot positively assert; nor would it in the least diminish the merit of their observations, if future experiments should prove them to have been mistaken, as to the simplicity of those substances. This remark should not be confined to later discoveries; it may as justly be applied to those earths and metals with which we have been long acquainted.

With regard to the metals, we have seen how little dependance is to be placed on specific gravities. A contrary anomaly to that which operates upon platina and mercury, may take place in others; and they may become as much heavier than the mean, as the former become lighter. In this state of union, they may for a long time appear homogeneous, even by the test of chemical reagents. One of the properties that renders metallic substances so precious is, their easy formation into such instruments as our necessities require. The fragile metals are but of secondary consequence; and, at most, serve to confer on those which are ductile, some quality which adapts them better to particular purposes. It often happens that, by being alloyed, two ductile metals become fragile; but we have no instance of the contrary effect in any high degree. It is therefore more to be supposed that we should look to simplification among the fragile metals; and, even at this early period, it may not be too

speculative, to consider the metallic bodies in an order which may bring together those which possess the greatest number of similar characters.

As an instance of this approximation, it may be observed, that nickel and cobalt strongly participate in the properties of copper and iron. The two former metals were long regarded as mixtures; and the doubts of the ancient chemists, who feared to pronounce as to their nature, may still be proved to have more foundation in truth than the assertion of the moderns, who have declared them to be simple. Acted upon by the same menstrua, forming insoluble compounds with the same acids, and soluble alike in other substances, they have but one or two marked properties that lead us to consider them as distinct metals. But palladium has at least five or six characters, as strong as those of any metal whatsoever, that distinguish it, not only from its elements, but also from all other metals.

Among the earths, this approximation is still more apparent. A leading character of these substances is, their tendency to enter into saline combinations, in which they receive new properties, and perform new functions. If we rank them according to this general tendency, we shall have the following order: barytes and strontia; lime and magnesia; glucine and alumina; zircon and silica. And, if we consider them two by two in this order, which is a natural one, we shall bring together precisely those which differ by the smallest number of chemical characters.

This investigation might be pursued still further; but we must wait the result of experiments: a wide field is open for research. In the dark ages of chemistry, the object was, to rival nature; and the substance which the adepts of those days were

busied to create, was universally allowed to be simple. In a more enlightened period, we have extended our enquiries, and multiplied the number of the elements. The last task will be to simplify; and, by a closer observation of nature, to learn from what a small store of primitive materials, all that we behold and wonder at was created.

XIII. *An Account of the sinking of the Dutch Frigate* Ambuscade, *of 32 Guns, near the Great Nore; with the Mode used in recovering her. By Mr.* Joseph Whidbey, *Master Attendant in Sheerness Dock Yard. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read April 28, 1803.

At eight o'clock in the morning of the 9th day of July, 1801, the Dutch frigate Ambuscade left the moorings in Sheerness harbour, her fore-sail, top-sails, and top-gallant-sails being set, with the wind aft, blowing strong. In about thirty minutes, she went down by the head, near the Great Nore; not giving the crew time to take in the sails, nor the pilot or officers more than four minutes notice, before she sunk; by which unfortunate event, twenty-two of the crew were drowned.

This extraordinary accident was owing to the hawse-holes being extremely large and low, the hawse-plugs not being in, and the holes being pressed under water by a crowd of sail on the ship, through which a sufficient body of water got in, unperceived, to carry her to the bottom.

The instant she sunk, she rolled over to windward across the tide, and lay on her beam ends; so that, at low water, the muzzles of the main deck guns were a little out of the water, and pointed to the zenith, with 32 feet of water round her.

The first point I had to gain, was to get her upright. Before

could accomplish it, I was obliged to cut away her fore-mast, nd main-top-mast; which had no effect, until the mizen-mast vas also cut away; she then instantly lifted her side, so that, t low water, the lee railing on the quarter deck was visible.

By proceeding in this manner, the first part of my object was btained, with a secured main-mast, and all its rigging, to enable ne, should I be fortunate enough to weigh the ship, to lighten ier by it with the greatest possible expedition.

The ship being in the forementioned state, gave me an opportunity, the next low water, to get out her quarter, forecastle, nd some of her main-deck guns, with a variety of other rticles.

I next proceeded to sling her; which was done with two nineeen-inch cables, divided into eight equal parts. The larboard ide of the ship being so much higher than the starboard, nabled me to clench each of the ends round two of the ports, xcepting one that was clenched round the main-mast; and, vith great difficulty, by long rods and diving, I got small lines ove through four of the ports on the starboard side, by which neans, I got four of the cables through those ports across her leck, which were clenched to the main-mast and larboard side, aving four ends on each side completely fast, at equal disances from each other. I brought the Broederscarp, of 1063 tons burthen, out of the harbour, which received the four ends on the starboard side; also four lighters, of 100 tons each, which took in the other four ends, on the larboard side, over their bows. All the eight ends were at low water hove down with great power, by a purchase lashed distinctly on each of them. I then laid down two 13-inch cables, spliced together, with an anchor of

24 *cwt.* in a direction with the ship's keel. On the end of the cable next the frigate a block was lashed, through which was rove a 9-inch hawser, one end of which was made fast to the ship; the other end was brought to a capstan on board the Broederscarp, and hove on it as much as it would bear, with an intention to relieve the frigate from the powerful effect of cohesion. This had so far the desired effect that, at about half flood, I perceived the ship to draw an end, and swing to the tide; and all the slings were considerably relieved. At high water, she was completely out of her bed. At the next low water, I hove all the purchases down again. At half flood she floated; and the whole group drove together into the harbour, a distance of three miles, and grounded the frigate on the west side of it. It took me two tides more to lift her on the shore, sufficiently high to pump her out; which was then done with ease, and the ship completely recovered, without the smallest damage whatever, either to her bottom or her sides.

I do not apprehend there is any thing new in the mode I adopted in weighing the Ambuscade, excepting the idea of removing the effect of cohesion, by the process before described; and I have every reason to think, that if that principle had been acted on, in the attempt made to weigh the Royal George, it would have succeeded.

I am, &c.

EXPLANATION OF THE FIGURES.

Plate V. Represents the Ambuscade Dutch frigate, as weighed from the east end of the Middle Sand, near the Great Nore, on the 17th of July, 1801.

Fig. 1. Broederscarp Dutch hulk.

Fig. 2. Chain lighter.

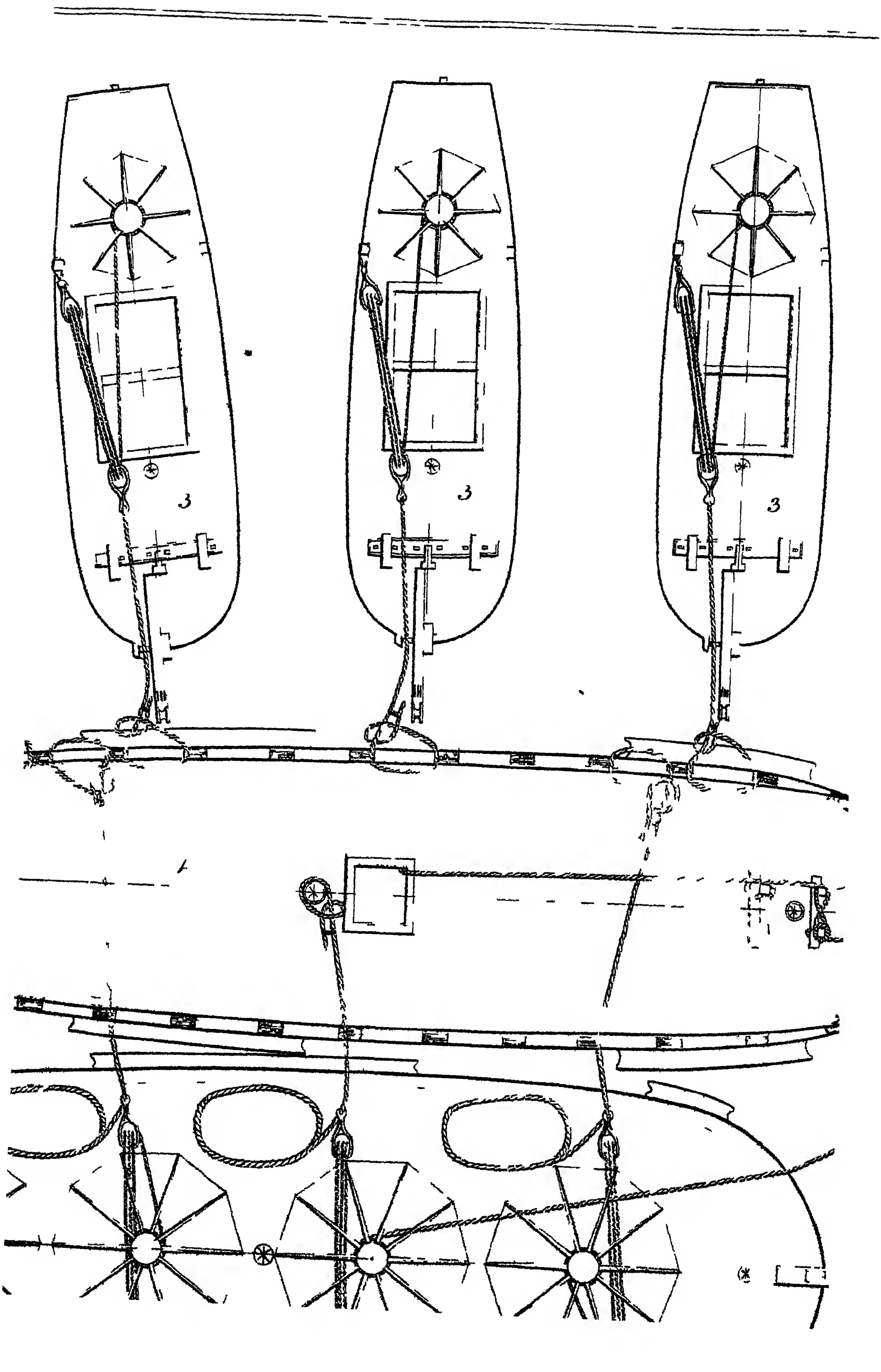
Fig. 3. Goodwill, Medway, and Sheerness, sailing lighters.

Fig. 4. The frigate Ambuscade.

Fig. 5. Section of ditto, showing her position, with the height of water, as marked by the dotted line, at low water; the tide rising sixteen feet.

Fig. 6. Cohesion cable, of 13 inches; 200 fathoms in length.

Fig. 7. Anchor of 24 *cwt.*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XIV. *Observations on a new Species of hard Carbonate of Lime; also on a new Species of Oxide of Iron. By the Count de* Bournon, *F. R. S. and L. S.*

Read May 26, 1803.

CARBONATE OF LIME.

A PARTICULAR investigation which I undertook to make, of the immense variety of forms presented to us by the carbonate of lime, (many of which have never yet been described,) having led me to observe more accurately some specimens I had before examined, my attention was forcibly attracted by a group of hexaedral pyramidal crystals, which are in Mr. GREVILLE's noble collection. As the exterior appearance of these crystals was different from that which is peculiar to carbonate of lime, I was desirous to ascertain their nature, and therefore endeavoured to bring them, by splitting, into the primitive rhomboidal form of this substance; but, upon making the attempt, I perceived, to my great astonishment, not only that I was not able to obtain any fracture that could possibly belong to the planes of the primitive rhomboid, but also, that the substance strongly resisted every attempt I made to procure a fracture in any other direction. In fact, every fracture I could obtain was irregular, and possessed more or less of that vitreous appearance which is peculiar to the greater number of hard stones. So remarkable was the hardness of this substance, that, although the points of the crystals were very thin, and consequently might

be supposed very brittle, yet they resisted my efforts to break them, in a very remarkable manner.

The resistance above spoken of appearing to me analogous to that of hard stones, I resolved to examine, with the most scrupulous attention, every thing relating to the peculiar characters of this substance.

Its hardness is very superior to that of common carbonate of lime, being such as to scratch very easily the fluate of lime; and, when rubbed with force upon glass, it takes off the polish of its surface, and sometimes leaves scratches upon it.

Its specific gravity, I found to be 2912.

This substance, of which I have since had an opportunity of observing a great number of specimens, I have always found to be without colour; and its crystals are very often perfectly transparent.

When powdered, and thrown upon a piece of iron heated nearly to redness, in a place that is perfectly dark, it occasions a very weak phosphorescent white light; this light is only sufficient to mark the place where the powder is thrown.

Its lustre is much greater than that of common carbonate of lime.

When put into nitric acid, a violent effervescence is produced; and it is very quickly dissolved, without leaving the smallest residuum.

Although this substance strongly resists any effort made to divide it by splitting, yet it shows a tendency to admit of being divided more easily in two directions, which would produce a rhomboidal tetraedral prism. I succeeded indeed, at last, though with great difficulty, and after many fruitless attempts, in procuring from it a perfect rhomboidal tetraedral prism, the angles

of which measured 128° and 52. (See Plate VI. Fig. 1.) I found it, however, impossible to obtain any smooth and even fractures on the terminal faces of this prism.

In some crystals, which were situated at the base of those which constituted the largest of the groups I examined, I observed perfectly-formed hexaedral prisms, which appeared to me to have been occasioned by the two angles of 52° (belonging to the rhomboidal tetraedral prism of 128° and 52°, which I have just described,) having been replaced; consequently, there were now two edges of 128°, and four others of 116°. See Fig. 2.

The principal crystals which form the above group, although they are about three inches in length, do not show themselves distinctly, except at their upper extremity, being joined together, and entangled with each other, throughout the rest of their length. The upper extremity just mentioned, is a very sharp hexaedral pyramid, as in Fig. 3. The solid angle of its summit, taken upon two of the opposite sides, is of 15°; and the sides of the pyramids, meeting together, form two angles of 128° each, and four others of 116°, as we have already seen to be the case with respect to the sides of the hexaedral prism; consequently, the base of this pyramid is an irregular hexagon.

Although the summit of this pyramid is sometimes formed in the manner above described, by the meeting of all its planes at the same point, (as is seen in Fig. 3,) yet it more frequently happens that the summit terminates in a ridge; the pyramid is then of a cuneiform shape, on account of the extension (which is sometimes very considerable) of two of its opposite sides, at the expense of the others, as in Fig. 4. Very frequently, indeed, the abovementioned extension is such as to cause the pyramids to be extremely thin; they then appear in the form of a very

acute isosceles triangle, the summit of which is truncated, and the sides of which are slightly bevelled. The bevel, however, is often so inconsiderable as to be scarcely perceptible. See Fig. 5.

Each of the two sides which, in those pyramids, acquire the abovementioned extension of surface, at the expense of the two others, has constantly appeared to me to belong to one of the sides which forms the angle of 128°, but taken in an opposite direction, as is represented in Fig. 6, which is supposed to be the base of one of them. This base is perfectly similar, in the measure of its angles, to that of the hexaedral prism of Fig. 2; nevertheless, this hexaedral prism sometimes appears also to have the two planes, which have replaced the edges of 52°, more extended than the four others.

In those crystals which are the most detached from each other, and exhibit a larger portion of their extent, it may be distinctly perceived, that the above pyramid is situated upon a hexaedral prism, of the same dimensions as the base of the pyramid; (see Fig. 7;) but, as the angle formed by the junction of the sides of the prism with those of the pyramid is extremely obtuse, (its measure being 172° 30',) the exact point of union of the prism with the pyramid cannot easily be distinguished.

The summit of the pyramid is sometimes replaced by two trapezoidal planes, situated, when the pyramid is of a cuneiform shape, on the broadest sides. These planes meet together, at the summit, in a ridge of 110°, and form, with the sides of the pyramid, on which they incline, an angle of 132° 30'. See Fig. 8.

At other times, this summit is replaced by two planes situated differently from the above, being placed on one of the edges contiguous to each of the two broad sides of the pyramid, and in opposition to each other. These planes, which are irregular

pentagons, meet together at the summit, in a ridge which is perpendicular to the axis, and form, with the edges of the pyramids, on which they incline, an angle of 140°, as in Fig. 9. These planes are sometimes very distinctly striated, the striæ being directed towards the summit; but, by following these striæ over the whole surface of the crystal, it evidently appears, that they are occasioned by the aggregation of a greater or smaller number of very thin crystals, which are united by the broadest sides of their pyramids.

Many of the crystals exhibit this variety combined with that represented in Fig. 8, as is seen in Fig. 10. The summit of the crystal is then terminated by a small tetraedral pyramid.

It frequently happens, that there exists only one of the planes of the diedral summit, represented in Fig. 9. It nevertheless intercepts the summit of the pyramid, which then becomes terminated by a hexagonal plane, inclined upon one of the edges contiguous to one of the broad sides, in such a way as to form with it an angle of 140°. (See Fig. 11.) These crystals are very often so considerably flattened, as to have nearly the appearance of very sharp isosceles triangular laminæ, which have their summits truncated, in such a manner as to form with one of the broad sides an angle of 140°, as in Fig. 12. I have seen crystals of this variety, the thickness of which scarcely exceeded that of a sheet of paper; yet, notwithstanding their thinness, I found that they might easily be handled, without danger of being broken.

Lastly, the summit of the hexaedral pyramid is sometimes terminated by a plane that is perpendicular to its axis. But I have never seen this last variety, except in combination with that represented in Fig. 9. This combination is shown in Fig. 13.

The above are all the forms I have been able to discover, in the species of carbonate of lime here described.

This substance does not appear to be very scarce. Among the crystallized carbonates of lime preserved in Mr. GREVILLE'S collection, I have met with about a dozen specimens of it, most of which came either from Carinthia, or from Transylvania, or from Scotland. The beautiful and delicately white stalactitical substance, hitherto known by the name of *flos ferri*, generally belongs to the substance here described, particularly certain pieces of it, which have their ramifications covered with small brilliant asperities, giving them the appearance of fine satin. These little asperities, all of which are inclined, in the same direction, to the axes of the various ramifications, are in fact so many very perfect but minute crystals, which most commonly belong to the forementioned flat pyramidal varieties.

Among the specimens of this kind of carbonate of lime which came from Carinthia, there exist some, in which the sharp pyramids are very small, and appear as if planted almost perpendicularly in the matrix. These specimens may, from the above circumstance, be more easily confounded with the common carbonate of lime in small needle-like crystals; there is, however, the following difference between them, namely, that in the common carbonate of lime, we cannot touch these little crystals, though ever so lightly, without breaking them; whereas, in the substance here described, the crystals are capable of resisting a tolerably strong compression of the fingers, and, if the pressure be increased, they very frequently, instead of breaking off, actually penetrate into the skin. The lustre of the latter substance is also much more lively than that of the former.

Another circumstance which might prevent our recognizing, at the first view, the crystals of this substance, when placed among those of common carbonates of lime, is, that the crystals of the latter substance are sometimes found in the form of a hexaedral pyramid, nearly as acute as that of the crystals above described; but, in that case, they break with the greatest facility, and the fractures are always smooth, and in the direction of the planes of the primitive rhomboid; a circumstance that is never observed in the crystals of the hard carbonate.

The matrix of this substance, in most of the specimens I have seen, is a brown oxide of iron, mixed with a portion of argill, and also with a considerable number of calcareous particles. In some of these may be observed the primitive rhomboid of the common carbonate of lime, grouped upon the crystals of the substance here treated of.

It may perhaps be questioned, whether the hard carbonate of lime I am now describing, (the mineralogical characters of which seem so much at variance with the chemical ones,) ought not to be referred to that kind which mineralogists have been already obliged to separate from the others, under the name of Arragonite, or whether it ought to be considered as different from the latter substance, and forming an additional new species, among the combinations of the carbonic acid with lime. It appears to me very difficult to determine the above question. The primitive crystal which I obtained from it, is not sufficiently perfect to serve as an accurate criterion; for, as we have seen, that crystal, which is a rhomboidal-tetraedral prism, cannot be divided according to its basis, that is, in one of its three natural directions; and this is exactly similar to what happens with respect to the primitive crystal obtained from the Arragonite. I confess, however, that if I were obliged to adopt an opinion, I should

be inclined to consider these two substances as distinct from each other.

I shall now take a short view of the analogy, and of the difference, existing in the characters of the two substances just mentioned.

Their specific gravity is nearly the same. The Abbé HAUY states that of the Arragonite at 2946. I found that of the hard carbonate of lime to be 2912.

The hardness of the latter is rather greater than that of the former: it scratches the Arragonite, but is not scratched by it.

The Arragonite is seldom found without a tinge, more or less considerable, of purple; but I have never observed any appearance of colour in the hard carbonate of lime.

The Arragonite, when thrown upon a heated iron, emits a very brilliant phosphorescent light, of a yellowish-orange colour. The other substance, when treated in the same manner, produces only a white phosphorescent light, scarcely perceptible.

From the Arragonite is obtained, by splitting, a rhomboidal tetraedral prism, of 116° and 64°; and, from the hard carbonate of lime, one of 128° and 52°.

The last-mentioned substance, in its secondary forms, passes into a hexaedral prism, which has two edges of 128°, and four others of 116°. Whereas, the Arragonite becomes a hexaedral prism only in consequence of the union of several of its rhomboidal prisms; and as, in that state, three of the edges of the prism are formed by the union of the edges of 64°, belonging to the tetraedral prism, it has three angles of 116°, and three others of 128°. This gives 732°, for the whole measure of the angles of the prism; which measure is too great by 12°; and is the cause that, in the formation of the prism, the rhomboids of which it is composed, are obliged sometimes to penetrate

each other; at other times, to form a re-entering angle, at the angles of 128°.

In another secondary form assumed by the Arragonite, there is a diedral summit with isosceles triangular planes, which are inclined upon the edges of 64°, belonging to the tetraedral prism, and meet together in a ridge of 110°. I have not perceived any trace of this form, in the kind of carbonate of lime treated of in this Paper; nor have I perceived the smallest trace of the pyramidal forms of the last-mentioned substance, among the crystals of the Arragonite.

Neither the hard carbonate of lime nor the Arragonite exhibit, by their chemical analysis, any signs of the cause that occasions them to differ from the common carbonate of lime. The Arragonite has been very carefully analysed by MM. KLAPROTH, VAUQUELIN, and THENARD; but their analyses did not show that it differed, in the smallest degree, from the common carbonate of lime. I desired Mr. CHENEVIX to be so good as to analyse the hard carbonate of lime; but his result was not more satisfactory. The cause of its difference from common carbonate of lime remains still undiscovered; yet there certainly exists a considerable difference between the two substances, and one which even the chemist is compelled to admit, when he takes the hard carbonate of lime into his hands, and which becomes still more evident to him, the moment he begins to reduce it into powder. It is however very clear, that the said difference arises from a cause which has hitherto eluded the investigations of chemistry. It may perhaps be supposed, that it is owing to the constituent particles of the substance being more closely connected; and this indeed might be sufficient to occasion a greater degree of specific gravity, and of hardness. But, in the first place, it may be asked what can produce this more intimate connection,

of which we find no other example in the immense quantity of crystallized carbonate of lime that has been hitherto examined, however perfect the form of the crystals, or however great their degree of transparency. In the second place, why should the above-mentioned cause change the character of the crystalline forms of carbonate of lime, when we see no instance of such a change in the crystalline forms of other substances, even when they show, by an increase in their hardness, in their specific gravity, and in their transparency, that a closer connection between their constituent particles has actually taken place.

It appears therefore impossible, in the present state of our knowledge, to determine the cause of the very great difference that exists between the common carbonate of lime and the hard kind here treated of; yet it is equally impossible not to be sensible, that the said difference is of such a nature as absolutely prevents us from considering them as of the same species. Whether it is right to join the hard carbonate of lime with the Arragonite, is a question I have already in some measure discussed, but respecting which, until I have had further opportunities of investigating the matter, I shall not venture to give a decided opinion.*

* I have lately received, from my worthy friend M. GILLET DE L'AUMONT, some imperfectly formed and colourless crystals of hard carbonate of lime, which, he says, were found inclosed in lava, near Vertzou, in the environs of Puy de Dome, in Auvergne, and which were considered as a kind of Arragonite. These crystals appear to me very similar to the hard carbonate of lime herein described; and M. GILLET informs me, that the above is not the only part of Auvergne in which these crystals are found in old lava. I remember perfectly well, that when I was examining the volcanic products of that province, on the spot, and also those of Velay, of Vivarais, and of Forez, I observed, in many of the lavas of the extinct volcanos of those provinces, groups of thin diverging crystals of carbonate of lime, which appeared to me much harder than crystals of common carbonate of lime in similar circumstances, so that I found it very easy to preserve them entire. I think it very probable, that the *Arragonite cylindroide* of the Abbé HAUY ought to be referred to this substance.

CUBIC OXIDE OF IRON.

Amongst the various examples which mineralogy, when studied with attention, constantly offers to us, of several different species being included in the combination of the same modifying substance with the same base, iron may be considered as affording one of the most striking. This metal, in its combinations with oxygen, varies considerably; insomuch that it presents us with several different species, according as the proportion of oxygen in the combination is increased. Thus, by the first degree of oxidation in which this metal offers itself to our notice, is formed the very attractable oxide of iron, which crystallizes in regular octaedrons. By the second degree of oxidation, in which there is a greater proportion of oxygen, a different oxide is produced, which is much less attractable than the former, and crystallizes in the form of a slightly acute rhomboid. Lastly, in the third degree of oxidation, in consequence of a still greater proportion of oxygen, the attractable property no longer exists; the power of crystallizing entirely ceases; and we have the ores called hæmatites, and the other earthy oxides, for instance, the brown, the red, the yellow, and the black, (the appearance of which latter very much resembles that of a bitumen,) between which there certainly exists some real chemical difference, that will probably hereafter be discovered.

The distinction of species here spoken of will perhaps appear extraordinary, to those persons who are accustomed to consider the combination of oxygen with iron as forming of itself a species in the genus, (which genus is determined by the nature of the metal, namely, iron,) because it may appear to them like dividing into various species, that which merely constitutes a single one. But I must observe, that in mineralogy it is not

merely the chemical combination of a particular acid with a particular basis which forms the species, but the mode in which that acid is combined with the basis. Perhaps, in many cases, the formation of the species may depend upon the introduction of a third principle, which, either from its mode of combination or from its nature, has hitherto eluded the investigations of chemistry. Thus, in the analysis of two plants, or of two animals, of totally different species, chemistry, in most cases, is not able to discover any thing but the same ingredients combined with each other. In these instances, therefore, it is evident that the mode of combination, and not the combination itself, is what determines the species.

The science of mineralogy is indebted to the Abbé HAUY, for having ascertained the primitive form of the slightly attractable oxide of iron, formerly known by the name of specular iron ore, to which he has given the name of *fer oligiste*. That form was supposed to be derived from the cube; but the Abbé HAUY, directed thereto by the secondary crystals, has shown that it belongs to a rhomboid of 87° and 93°. Nevertheless, the cube must not be excluded from among the forms belonging to the oxides of iron. On the contrary, it constitutes a particular species, which has hitherto been entirely overlooked by mineralogists.

Between the slightly attractable oxide of iron, (or specular iron ore,) and that kind which no longer crystallizes, except in a very indeterminate form, nature has placed another species, the surface of which is of a gray colour, and has a specular appearance, pretty much like the iron ore from the island of Elba. This kind is not at all acted upon by the magnet; and seems to be in the last degree of oxidation in which iron retains the property of crystallizing in a regular form.

Its form is a perfect cube, the edges or solid angles of which are sometimes replaced by small planes.

Its fracture is conchoidal: it has a smooth grain, with a small degree of lustre; and, although it is impossible to make a regular fracture in any particular direction, yet the fracture shows that the crystalline laminæ, or collection of molecules, are situated on the surface of the cube.

Its hardness is rather inferior to that of the slightly attractable oxide of iron.

Its specific gravity is very low; I found it to be only 3961.

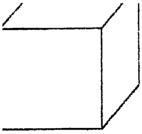
Its powder is more red than that of the slightly attractable oxide of iron, but has not the yellow cast observed in the powder of the hæmatite.

To this species ought to be referred the *eisen-glimmer* of the Germans, when it is not attractable: when, on the contrary, it is attractable, it belongs to the slightly attractable oxide of iron. In the first case, this *eisen-glimmer* is in small laminæ, which are very brilliant, but of an indeterminate form; and it frequently is found accompanying the hæmatites, and having the abovementioned appearance. If, in the formation of the hæmatites, some particles of the oxide of iron of which they are composed happen to contain a rather smaller proportion of oxygen, they naturally become the cubic oxide of iron here treated of. Indeed, some hæmatites, although they are crystallized in a very indeterminate manner, and are really of a species different from the cubic oxide of iron, show, by the colour and brilliancy of their surface, a tendency to approach towards it.

When the octaedral very attractable oxide of iron (the *fer oxidulé* of the Abbé Haüy) is in irregular and confused masses, the cubic oxide of iron here described is sometimes found mixed with it; and, in that case, it renders the ore less sensible to the

influence of the magnet, in proportion to the quantity of it contained in the mixture. Its presence may be easily detected, when the ore is pounded, by the appearance of a red powder, which shows itself in the middle of the black powder of the aforesaid ore.

Among the specimens of iron ore from Gellivare, in Swedish Lapland, which were brought here by Mr. SWEDENSTIERNA, a very intelligent Swedish mineralogist, there were some, in which this cubic oxide of iron was so pure and unmixed, that they were not at all, or at least in a very slight degree, acted upon by the magnet; but, that they really belonged to the species here treated of, might be plainly perceived, by pretty strongly marked striæ upon the surface, which crossed each other at right angles, and were sometimes even seen within the substance. In other specimens, the cubic oxide of iron was mixed, in greater or less proportion, with the octaedral kind; and this latter became, in that case, less attractable, in proportion as the former species was more abundantly mixed with it. Those who have been accustomed to examine this species, and are in the habit of using a lens, (without which many interesting objects in mineralogy escape our observation,) may become capable of distinguishing it in the ore; and this faculty may perhaps hereafter be of great importance, provided it should be found that the quality of the iron obtained from the ore, is affected by the abovementioned mixture. If we scratch with a knife such specimens as contain both the abovementioned species, we may, indeed, by the appearance of a red powder, discover the particles of the cubic oxide, (which particles, if separated, would not be attractable,) and we may thereby, in some measure, estimate the proportion of the above oxide in the ore.



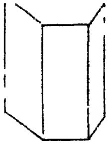
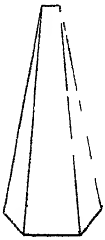


Fig. 4



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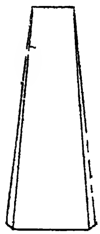

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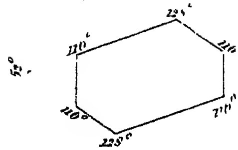

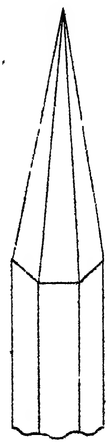

Fig. 8

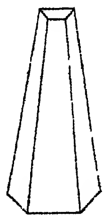

Fig. 9

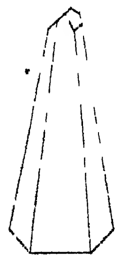

Fig. 10.

Fig. 11

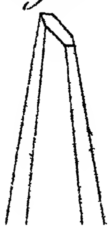

Fig. 12.

Fig. 1

XV. *Account of the Changes that have happened, during the last Twenty-five Years, in the relative Situation of Double-stars; with an Investigation of the Cause to which they are owing. By* William Herschel, *LL. D. F. R. S.*

Read June 9, 1803.

In the Remarks on the Construction of the Heavens, contained in my last Paper on this subject,* I have divided the various objects which astronomy has hitherto brought to our view, into twelve classes. The first comprehends insulated stars.

As the solar system presents us with all the particulars that may be known, respecting the arrangement of the various subordinate celestial bodies that are under the influence of stars which I have called insulated, such as planets and satellites, asteroids and comets, I shall here say but little on that subject. It will, however, not be amiss to remark, that the late addition of two new celestial bodies, has undoubtedly enlarged our knowledge of the construction of the system of insulated stars. Whatever may be the nature of these two new bodies, we know that they move in regular elliptical orbits round the sun. It is not in the least material whether we call them asteroids, as I have proposed; or planetoids, as an eminent astronomer, in a letter to me, suggested; or whether we admit them at once into the class of our old seven large planets. In the latter case, however, we must recollect, that if we would speak with precision,

* See Phil. Trans. for 1802, p. 477.

distance, of double stars, as will be reported; and, in order to make the required principles very clear, I shall give them in a few short and numbered sentences, that they may be referred to hereafter.

In Plate VII. Fig. 1, let us call the place of the sun, which may also be taken for that of the observer, O. In the centre of an orbit or plane N F S P is α Geminorum; and, if any other star is to be examined, we have only to exchange the letter α for that by which such double star is known. This letter is always understood to represent the largest of the two stars which make up the double star; and a general expression for its smaller companion will be x. N, F, S, P, represent the positions of the different parts of the heavens, with respect to α, north, following, south, and preceding; and the small letters n, f, s, p, stand for the same directions with respect to O. $x \alpha P$, is the angle of position of the two stars x and α, with the parallel F P.

As the motion of an observer affects the relative situation of objects, we have three bodies to consider, in our investigation of the cause of the changes which will be pointed out; the sun, the large star, and the small star, or, as we have shortly called them, O, α, x. This admits of three cases: a motion of one of the three bodies; another, of two; and a third, of all the three bodies together. We shall now point out the consequences that will arise in each of the cases.

Single Motions.

No. 1. Motion of x. When α and O are at rest, the motion of x may be assumed, so as perfectly to explain any change of the distance of the two stars, and of their angle of position.

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Single Motions.

No. 1. Motion of x. When α and O are at rest, the motion of x may be assumed, so as perfectly to explain any change of the distance of the two stars, and of their angle of position.

No. 2. Motion of α. When x and O are at rest, and α has a motion, either towards P, N, F, or S, then the effect of it, whatever may be the angle P α O, will be had by entering the following Table, with the direction of the given motion.

Motion.	Distance.	Angle.	Quadrants.
αP	− +	+ −	1st and 4th 2 —— 3
αF	+ −	− +	1 —— 4 2 —— 3
αN	− +	− +	1 —— 2 3 —— 4
αS	+ −	+ −	1 —— 2 3 —— 4

No. 3. Motion of O. 1st case. When α and x are at rest, and the angle P α O is 90 degrees, a proper motion of O, towards either *p*, *f*, *n*, or *s*, which will be extremely small when compared with the distance of O from α, can have no effect on the apparent distance, or angle of position, of the two stars; and therefore no other motion, composed of the directions we have mentioned, will induce a change in the comparative situation of α and x.

2d case. When the plane PNFS is oblique to the ray αO, and the angle PαO more than 90 degrees, the effect of the motion of O will be had by the following Table.

Motion	Distance	Angle.	Quadrants.
O*p*	+	−	1st and 2d 3 — 4
O*f*	−	+	1 — 2 3 — 4
O*n*	±	±	1 — 3 2 — 4
O*s*	∓	∓	1 — 3 2 — 4

3d case. When the angle PαO is less than 90 degrees, the following Table must be used.

Motion	Distance.	Angle.	Quadrants.
O*p*	−	+	1st and 2d 3 — 4
O*f*	+	−	1 — 2 3 — 4
O*n*	∓	∓	1 — 3 2 — 4
O*s*	±	±	1 — 3 2 — 4

Double Motions.

No. 4. If we admit different motions in two of our three bodies, and if the ratio of the velocities, the directions of the

motions, and the ratio of the distances of the bodies be given quantities, a supposition in which we admit their concurrence, may explain the phenomena of a double star, but can never be probable.

Motions of the three Bodies.

No. 5. If we admit different motions in every one of the three bodies, O, α, x, and if the velocities and directions of the motions, as well as the relative distances of the three bodies are determined, an hypothesis which admits the existence of such motions and situations, may resolve the phenomena of a double star, but cannot have any pretension to probability.

The compass of this Paper will not allow me to give the observations of my double stars at full length; I shall therefore, in the examination of every one of them, only state those particulars which will be required for the purpose of investigating the cause of the changes that have taken place, either in the distance, or angle of position, of the two stars of which the double star is composed.

As the arguments in the case of most of these stars will be nearly the same, it may be expected, that the first two or three which are to be examined will take up a considerable space; and the number of double stars, in which I have already ascertained a change, amounting to more than fifty, it will not be possible to give them all in one paper; I shall therefore confine the present one to a moderate length. and leave it open for a continuation at a future opportunity.

α Geminorum.

From my earliest observations on the distance of the two stars which make up the double star in the head of Castor, given in the first of my catalogues of double stars, we find, that about 23 years and a half ago, they were nearly two diameters of the large star asunder. These observations have been regularly continued, from the year 1778 to the present time, and no alteration in the distance has been perceived: the stars are now still nearly 2 diameters of the large one asunder.

It will be necessary to enter a little into the practicability of ascertaining distances by a method of estimation apparently so little capable of precision. From a number of observations and experiments I have made on the subject, it is certain that the apparent diameter of a star, in a reflecting telescope, depends chiefly upon the four following circumstances: the aperture of the mirror with respect to its focal length; the distinctness of the mirror; the magnifying power; and the state of the atmosphere at the time of observation. By a contraction of the aperture, we can increase the apparent diameter of a star, so as to make it resemble a small planetary disk. If distinctness should be wanting, it is evident that the image of objects will not be sharp and well defined, and that they will consequently appear larger than they ought. The effect of magnifying power is, to occasion a relative increase of the vacancy between two stars that are very near each other; but the ratio of the increase of the distance is not proportional to that of the power, and sooner or later comes to a maximum. The state of the atmosphere is perhaps the most material of the four conditions, as we have it not in our power to alter it. The effects of moisture, damp air, and

haziness, (which have been related in a paper where the causes that often prevent the proper action of mirrors were discussed,) show the reason why the apparent distance of a double star should be affected by a change in the atmosphere. The alteration in the diameter of Arcturus, extending from the first to the last of the ten images of that star, in the plate accompanying the abovementioned paper,* shows a sufficient cause for an increase of the distance of two stars, by a contraction of their apparent disks. A skilful observer, however, will soon know what state of the air is most proper for estimations of this kind. I have occasionally seen the two stars of Castor, from $1\frac{1}{2}$ to 2 and $2\frac{1}{2}$ diameters asunder; but, in a regular settled temperature and clear air, their distance was always the same. The other three causes which affect these estimations, are at our own disposal; an instance of this will be seen in the following trial. I took ten different mirrors of 7 feet focal length, each having an aperture of 6,3 inches, and being charged with an eye-glass which gave the telescope a magnifying power of 460. With these mirrors, one after another, the same evening, I viewed the two stars of our double star; and the result was, that with every one of them, the stars were precisely at an equal distance from each other. These mirrors were all sufficiently good to show minute double stars well; and such a trial will consequently furnish us with a proper criterion, by which we may ascertain the goodness of our telescope, and the clearness of the atmosphere required for these observations. To those who have not been long in the habit of observing double stars, it will be necessary to mention, that, when first seen, they will appear nearer together than after a certain time; nor is it so soon as might be

* See Phil. Trans. for 1803, page 232, Plate III.

expected, that we see them at their greatest distance. I have known it to take up two or three months, before the eye was sufficiently acquainted with the object, to judge with the requisite precision.

Whatever may be the difficulties, or uncertainties, attending the method of determining the distance of two close stars by an estimation of the apparent diameter, it must however be confessed, that we have no other way of obtaining the same end with so much precision. Our present instance of α Geminorum, will show the degree of accuracy of which such estimations are capable, and at the same time prove, that the purpose for which I shall use the estimated interval between the two stars will be sufficiently answered. By an observation of the 10th of May, 1781, we have the diameter of the largest of the two stars to that of the smallest as 6 to 5; and, according to several measures I have taken with the micrometer, we may admit their distance, diameters included, to be five seconds. Then, as the vacancy between the two stars is nearly, but not quite, 2 diameters of the large one, I shall value it at $1\frac{7}{8}$. From this we calculate, that the diameter of the large star, under the circumstances of our estimation, is nearly $1'',35$: so that an error of one quarter of such a diameter, which is the most we can admit, will not exceed $0'',34$. Nor is it of much consequence, if the measure of $5''$ should not be extremely correct; as a small mistake in that quantity will not materially affect the error of estimation by the diameter, which, from what has been said, if the measure was faulty to a second, would not amount to more than one-fifteenth part of it.

Having thus ascertained that no perceptible change in the distance of the stars has taken place, we are now to examine

the angle of position. In the year 1779, it was 32° 47′ north preceding; and, by a mean of the three last measures I have taken, it is now only 10° 53′. In the space of about 23 years and a half, therefore, the angle of position has manifestly undergone a diminution, of no less than 21° 54′; and, that this change has been brought on by a regular and gradual decrease of the angle, will be seen when the rest of the measures come to be examined.

The accuracy of the micrometer which has been used, when the angles of position were taken, being of the utmost importance, it becomes necessary to ascertain how far it will be safe to rely on the result of the measures. It might be easily shown that, in the day time, a given angle, delineated on a card, and stuck up at a convenient distance, may be full as accurately measured by a telescope furnished with this micrometer, as it can be done by any known method, when the card is laid on a table before us; but this would not answer my purpose. For, objects in motion, like the stars, especially when at a distance from the pole, cannot be measured with such steadiness as those which are near us, and at rest. The method of illuminating the wires, and other circumstances, will likewise affect the accuracy of the angles that are measured, especially when the distance of the stars is very small. I shall therefore have recourse to astronomical observations, in order to see what the micrometer has actually done.

January 22, 1802. The position of A Orionis was taken. 1st measure, 52° 38′ south preceding; 2d measure, 54° 14′. Mean of the two measures, 53° 26′. Deviation of the measures from the mean, 48′.

March 4, 1802. 11 Monocerotis. 1st measure, 28° 18′ south

following; 2d measure, 26° 49′. Mean of the two, 27° 34′. Deviation from the mean, 45′.

February 9, 1803. *α* Geminorum. 1st measure, 6° 11′ north preceding; 2d measure, 4° 48′. Mean of the two, 5° 29′. Deviation from the mean, 41′.

September 6, 1802. *η* Coronæ. 1st measure, 89° 42′ north following; 2d measure, 89° 38′. Mean of the two, 89° 40′. Deviation from the mean, 2′.

When these observations are considered, we shall not err much if we admit that, in favourable circumstances, and with proper care, the micrometer, by a mean of two measures, will give the position of a double star true to nearly one degree; but, as the opportunities of taking very accurate measures are scarce, it will be necessary to have recourse to some more discordant observations.

February 18, 1803. *β* Orionis. 1st measure, 72° 58′ south preceding; 2d measure, 67° 24′. Mean of the two, 70° 11′. Deviation from the mean, 2° 47′.

But a memorandum to the observation says, that the evening was not favourable. We may therefore admit, that in the worst circumstances which can be judged proper for measuring at all, an error in the angle of position by two measures will not amount to three degrees.

It will be remarked, when we come to compare single measures which have been taken on different nights, that they are somewhat more discordant; but I have not ventured to reject them on that account, except in cases where it was pretty evident that some mistake in reading off, or other accident to which all astronomical observations are liable, was to be apprehended. Nor can such disagreements materially affect the

conclusions I have drawn, when it appears that the deviations happen sometimes to be on one side, and sometimes on the other side, of the true angle of position. For, since that angle is not a thing that will change in the course of a few nights, the excess of one measure will serve to correct the defect of another; and we are not to think it extraordinary, when stars are so near together, and their motion through the field of view (in consequence of the high magnifying power we are obliged to use) so quick, that we should now and then even fall short of that general accuracy which may be had by a careful use of the micrometer.

I shall now enter into an examination of the cause of the change in the angle of position of the small star near Castor.

A revolving star, it is evident, would explain in a most satisfactory manner, a continual change in the angle of position, without an alteration of the distance. But this, being a circumstance of which we have no precedent, ought not to be admitted without the fullest evidence. It will therefore be right to examine, whether the related phenomena cannot be satisfactorily explained by the proper motions of the stars, or of the sun.

Single Motions.

(a) The three bodies we have to consider, are O, α, and x; and, supposing them to be placed as they were observed to be in the year 1779; the angle $x\alpha$P, in Fig. 1, will be 32° 47′ north preceding. We are at liberty to let the angle PαO be what will best answer the purpose. Then, in order to examine the various hypotheses that may be formed, according to the arrangement of the principles we have given, we shall begin with No. 1; and, as this admits that all phenomena may be

resolved by a proper motion of x, let us suppose this star to be placed any where far beyond α, but so as to have been seen, in the year 1779, where the angle of position, 32° 47′ north preceding, and the observed distance, near 2 diameters of the large star, required it. With a proper velocity, let it be in motion towards the place where it may now be seen at the same distance from Castor, but under an angle of position only 10° 53′ north preceding. It may then be admitted, that a small decrease of the distance which would happen at the time when the angle of position was 21° 50′, could not have been perceived; so that the gradual change in the observed angle of position, as well as the equality of the distance of the two stars, will be sufficiently accounted for. But the admission of this hypothesis requires, that α Geminorum and the solar system should be at rest; and, by the observations of astronomers, which I shall soon have occasion to mention, neither of these conditions can be conceded.

(b) If, according to No. 2, we admit the motion of α, we shall certainly be more consistent with the observations which astronomers have made on the proper motion of this star;* and, as a motion of the solar system, which I shall have occasion to mention hereafter, has not been rigidly proved, it may, for the sake of argument, be set aside; nor has a proper motion of the star x been any where ascertained. The retrograde annual proper motion of Castor, in right ascension, according to Dr. MASKELYNE, is 0″,105. This, in about $23\frac{1}{2}$ years, during which

* See TOBIÆ MAYERI *Opera inedita. De motu fixarum proprio,* page 80. Also Dr. MASKELYNE's first Volume of Observations. Explanation and Use of the Tables, page iv. Or Mr. WOLLASTON's Astronomical Catalogue, end of the Preface. Likewise *Connoissance des Temps pour l'Année* VI. page 203. *Sur le Mouvement particulier propre a différentes Etoiles;* par Mons. DE LA LANDE.

time I have taken notice of the angle of position and distance of the small star, will amount to a change of nearly 2″,47. Then, if we enter the short Table I have given in No. 2, with the motion αP, we find, that in the first quadrant, where the small star is placed, the distance between the two stars will be diminished, and the angle of position increased. But since it appears, by my observations, that the distance of the stars is not less now than it was in 1780; and that, instead of an increase in the angle of position, it has actually undergone a diminution of nearly 22 degrees; it follows, that the motion of α Geminorum in right ascension, will not explain the observed alterations in the situation of this double star. If, according to Mr. DE LA LANDE's account,* we should also consider the annual proper motion of α in declination, which is given 0″,12 towards the north, we shall find, by entering our Table with the motion αN, amounting to 2″82, that the distance of the two stars will be still more diminished; but that, on the contrary, the angle of position will be much lessened; and, by combining the two motions together, the apparent disks of the two stars should now be a little more than one-tenth of a second from each other, and the angle of position 35 degrees south preceding. But, since neither of these effects have taken place, the hypothesis cannot be admitted.

(*c*) That the sun has a proper motion in space, I have shown with a very high degree of evidence, in a paper which was read at the Royal Society about twenty years ago.† The same opinion was before, but only from theoretical principles, hinted at by Mr. DE LA LANDE, and also by the late Dr. WILSON, of

* See page 211 of the treatise before referred to.

† See Phil. Trans. Vol. LXXIII. page 247.

Glasgow;* and has, since the publication of my paper, been taken up by several astronomers,† who agree that such a motion exists. In consequence of this, let us now, according to No. 3, assign to the sun a motion in space, of a certain velocity and direction. Admitting therefore α and x to be at rest, let the angle PαO be 90 degrees; then, by the 1st case of No. 3, we find that none of the observed changes of the angles of position will admit of an explanation. There is moreover an evident concession of the point in question, in the very supposition of the above angle of 90 degrees; for, if x be at the same distance as α from the sun, and no more than 5″ from that star, its real distance, compared to that of the sun from the star, will be known; and, since that must be less than the 40 thousandth part of our distance from Castor, these two stars must necessarily be within the reach of each other's attraction, and form a binary system.

(d) Let us now take the advantage held out by the 2d case of No. 3, which allows us to place x far behind α; in which situation, the angle PαO will be more than 90 degrees. The star x being less than α, renders this hypothesis the more plausible. Now, as a motion of Castor, be it real or apparent, has actually been ascertained, we cannot set it aside; the real motion of O, therefore, in order to account for the apparent one of α, must be of equal velocity, and in a contrary direction; that is, when decomposed, 0″,105 towards *f*, and 0″,12, towards *s*. The effect of the sun's moving from O towards *f*, according to

* See my note in Phil. Trans. Vol. LXXIII. page 283.

† See *Astronomishes Jahrbuch für das Jahr* 1786; *seite* 259. *Uber die fortrückung unseres Sonnen-Systems, von* Herrn *Professor* Prevost. *Und für das Jahr* 1805; *seite* 113.

the 1st Table in No. 3, is, that the distance between the two stars will be diminished, and the angle of position increased. But these are both contrary to the observations I have given. The motion of O in declination towards *s*, according to the same Table, will still diminish the distance of the two stars, but will also diminish the angle of position. Then, since a motion in right ascension increases the angle, while that in declination diminishes it, the small star may be placed at such a distance that the difference in the parallax, arising from the solar motion, shall bring the angle of position, in $23\frac{1}{2}$ years, from 32° 47′ to 10° 53′; which will explain the observed change of that angle. The distance of the star *x*, for this purpose, must be above $2\frac{1}{3}$ times as much as that of *α* from us. But, after having in this manner accounted for the alteration of the angle of position, we are, in the next place, to examine the effect which such a difference of parallax must produce in the apparent distance of the two stars from each other. By a graphical method, which is quite sufficient for our purpose, it appears, that the union of the two motions in right ascension and declination, must have brought the two stars so near, as to be only about half a diameter of the large star from each other; or, to express the same in measures, the centres of the stars must now be 1″,8 nearer than they were $23\frac{1}{2}$ years ago. But this my observations cannot allow; for we have already shown, that any change of more than 3 or 4-tenths of a second must have been perceived.

If, on the other hand, we place the star *x* at such a distance that the solar parallax may only bring it about 4-tenths of a second nearer to *α*, which is a quantity we may suppose to have escaped our notice in estimating the apparent distance of the two stars, then will the angle of position be above 20 degrees

too large. This shows, that no distance, beyond Castor, at which we can place the star, will explain the given observations.

(e) The last remaining trial we have to examine, is to suppose x to be nearer than α; the angle $P\alpha O$, will then be less than 90 degrees; and the effect of a motion of O towards *f*, by the 2d Table in No. 3, will be an increase of the distance of the two stars, and a diminution of their angle of position. But the motion Os, which is also to be considered, will add to the increase of the distance, and counteract the diminution of the angle. It is ther.fore to be examined, whether such an increase of distance as we can allow to have escaped observation, will explain the change which we know to have happened in the angle, during the last 23½ years. By the same method of compounding the two motions as before, it immediately appears, than we cannot place the small star more than about 1-tenth of the distance $O\alpha$ on this side of Castor, without occasioning such an increase of the apparent distance of the two stars as cannot possibly be admitted; and that, even then, the angle of position, instead of being less, will be a few degrees larger, at the end of 23½ years, than it was at the beginning. This hypothesis, therefore, like all the foregoing ones, must also be given up, as inconsistent with my observations.

It is moreover evident, that the observations of astronomers on the proper motion of the stars in general, will not permit us to assume the solar motion at pleasure, merely for the sake of accounting for the changes which have happened in the appearances of a double star. The proper motion of Castor, therefore, cannot be intirely ascribed to a contrary motion of the sun. For we can assign no reason why the proper motion of this star alone, in preference, for instance, to that of Arcturus,

of Sirius, and of many others, should be supposed to arise from a motion of the solar system. Now, if they are all equally intitled to partake of this motion, we can only admit it in such a direction, and of such a velocity, as will satisfy the mean direction and velocity of the general proper motions of the stars; and place all deviations to the account of a real proper motion in each star separately.

Double Motion.

(f) In order to explain the phenomena of our double star, according to No. 4, by the motion of two bodies, for instance α and x, it will be required that they both should move in given directions; that the velocities of their motions should be in a given ratio to each other; and that this ratio should be compounded with the ratio of their distances from O; a supposition which must certainly be highly improbable. To show this with sufficient evidence, let us admit that, according to the best authorities, the annual proper motion of Castor is $-0'',105$ in right ascension, and $0'',12$ in declination towards the north. Then, as the small star, without changing its distance, has moved through an angle of $21^\circ\ 54'$, the only difference in the two motions of these stars, will be expressed by the extent of the chord of that angle. To produce the required effect, it is therefore necessary that the motion of α, which is given, should regulate that of the small star, whose relative place at the end of $23\frac{1}{2}$ years is also given. Then, as α moves in an angle of $53^\circ\ 31'$ north preceding, and with a velocity which, being expressed by the space it would describe in $23\frac{1}{2}$ years, will be $3'',51$, it is required that x shall move in an angle of $29^\circ\ 25'$, likewise north preceding, and with a velocity of $3'',02$. The

ratio of the velocities, therefore, and the directions of the motions, are equally given. But this will not be sufficient for the purpose: their distance from O must also be taken into consideration. It has been shown, that the two stars cannot be at an equal distance from us, without an evident connection; it will therefore be necessary for those who will not allow this connection, to place one of them nearer to us than the other. But, as the motions which have been assumed, when seen from different distances, will subtend lines whose apparent magnitudes will be in the inverse ratio of the assumed distances, it is evident that this ratio, if the motions are given, must also be a given one; or that, if the distances be assumed, the ratio of the motions must be compounded with the ratio of the distances. How then can it be expected that such precise conditions should be made good, by a concurrence of circumstances owing to mere chance? Indeed, if we were inclined to pass by the difficulties we have considered, there is still a point left which cannot be set aside. The motion of the solar system, although its precise direction and velocity may still be unknown, can hardly admit of a doubt; we have therefore a third motion to add to the former two, which consequently will bring the case under the statement contained in our 7th number, and will be considered hereafter.

(g) If we should intend to change our ground, and place the two motions in O and x, it will then be conceded, that the motion of α is only an apparent one, which owes its existence to the real motion of the sun. By this, the effect of the solar parallax on any star at the same distance will be given; and it cannot be difficult to assume a motion in x, which shall, with the effect of this given parallax, produce the apparent motion, in the

direction of a chord from the first to the last angle of position pointed out by my observations; taking care, however, not to place the stars α and x at the same distance from us; and using the inverse ratio of the solar parallax as a multiple in the assigned motion. For instance, let the sun have a motion of the velocity expressed as before by 3″,51, and in a direction which makes an angle of 53° 31′ south following with the parallel of α Geminorum; and let the small star x have a real motion in an angle of 18° 40′ south preceding from the parallel of its situation, and with a real velocity which, were it at the distance of α, would carry it through 1″,89. Then, if the distance of the small star be to that of the large one as 3 to 2, the effect of the solar parallax upon it will be $\frac{2}{3}$ of its effect upon α; that is, while α, which is at rest, appears to move over a space of 3″,51, in an angle of 53° 31′ north preceding, the parallactic change of place in x will be 2″,34 in the same direction. This, though only an apparent motion, will be compounded with the real motion we have assigned to it, but which, at the distance of α, will only appear as 1″,26; and the joint effect of both will bring the star from the place in which it was seen 23½ years ago, to that where now we find it situated. α, in the same time, will appear to have had an annual proper motion of — 0″,105 in right ascension, and 0″,12 in declination towards the north; and thus all phenomena will be explained.

From this statement, we may draw a consequence of considerable importance. If we succeed, in this manner, in accounting for the changes observed in the relative situation of the two stars of a double star, we shall fail in proving them to form a binary system; but, in lieu of it, we shall gain two other points, of equal value to astronomers. For, as α Geminorum, according

to the foregoing hypothesis, is a star that has no real motion, its apparent motion will give us the velocity and direction of the motion of the solar system; and, this being obtained, we shall also have the relative parallax of every star, not having a proper motion, which is affected by the solar motion. Astronomical observations on the proper motion of many different stars, however, will not allow us to account for the motion of α Geminorum in the manner which the foregoing instance requires; the hypothesis, therefore, of its being at rest, must be rejected.

(h) If we place our two motions in O and α, we shall be led to the same conclusion as in the last hypothesis. The known proper motion of α, and the situations of the small star in 1779 and 1803, given by my observations, will ascertain the apparent motion of x, now supposed to be at rest. Then, since the change in the place of x must be intirely owing to the effect of parallax, it will consequently give us, in the same manner as before, the quantity and direction of the motion of the solar system, and the relative distances of all such stars as are affected by it. But, here again, the solar motion required for the purpose is such as cannot be admitted; and the hypothesis is not maintainable.

Motion of the three Bodies.

(i) There is now but one case more to consider, which is, according to No. 5, to assign real motions to all our three bodies; and this may be done as follows. Suppose the sun to move towards λ Herculis, with the annual velocity 1.

Let the apparent motion of α Geminorum be as it is stated in the astronomical tables before mentioned; but suppose it to arise from a composition of its real motion with the effect of the

systematical parallax, as we may call that apparent change of place of stars which is owing to the motion of the solar system. Let the real motion of x, aided by the effect of the same parallax, be the cause of the changes in the angle of position which my observations have given. We may admit the largest of the two stars of our double star to be of the second magnitude; and, as we are not to place x too near α, we may suppose its distance from O to be to that of α from the same as 3 to 2. In this case, O will move from the parallel of α, in an angle of 60° 37′ north following, with an apparent annual velocity of ,4536. The motion of α in right ascension, may be intirely ascribed to solar parallax; but its change of declination, cannot be accounted for in the same manner. Let us therefore admit that the solar velocity, in the direction we have calculated, will produce an apparent retrograde motion in α, which, in 23½ years will amount to 2″,085 in right ascension. But the same parallax will also occasion a change in declination, towards the south preceding, of 3″,701; and, as this will not agree with the observed motion of α, we must account for it by a proper motion of this star directly towards the north. The real annual velocity required for this purpose, must be 1,3925.

The apparent motion of x, by parallax, at the distance we have placed this star, will be 2″,832 towards the south preceding; and, by assigning to it an annual proper motion of the velocity 1,3854, in the direction of 73° 10′ north preceding its own parallel, the effect of the solar parallax and this proper motion together, will have caused the small star, in appearance, to revolve round α, so as to have produced all the changes in the angle of position which my observations have given; and, at the same time, α will have been seen to move from its former

place, at the annual rate of 0″,105 in right ascension, and 0″,12 in declination towards the north.

In this manner, we may certainly account for the phenomena of the changes which have taken place with the two stars of α Geminorum. But the complicated requisites of the motions which have been exposed to our view, must surely compel every one who considers them to acknowledge, that such a combination of circumstances involves the highest degree of improbability in the accomplishment of its conditions. On the other hand, when a most simple and satisfactory explanation of the same phenomena may be had by the effects of mutual attraction, which will support the moving bodies in a permanent system of revolution round a common centre of gravity, while at the same time they follow the direction of a proper motion which this centre may have in space, it will hardly be possible to entertain a doubt to which hypothesis we ought to give the preference.

As I have now allowed, and even shown, the possibility that the phenomena of the double star Castor may be explained by proper motions, it will appear that, notwithstanding my foregoing arguments in favour of binary systems, it was necessary, on a former occasion, to express myself in a conditional manner,* when, after having announced the contents of this Paper, I added, "*should these observations be found sufficiently conclusive;*" for, if there should be astronomers who would rather explain the phenomena of a small star appearing to revolve round Castor by the hypothesis we have last examined, they may certainly claim the right of assenting to what appears to them most probable.

I shall now enter into a more detailed examination of the

* See Phil. Trans. for 1802, page 486.

several angles of position I have taken at different times, and show that they agree perfectly well with the appearances which must arise from the revolution of a small star round Castor. A calculation of these angles may be had, by finding the annual motion of the small star, from the change of 21° 54′, which has been shown to have taken place in 23 years and 142 days Accordingly, I have given, in the 1st column of the following Table, the time when the angles were taken. In the 2d, are the angles as they were found by measure; they are all in the north-preceding quadrant. The 3d column contains a calculation from the annual motion of 56′,18, obtained as before mentioned: it shows what these angles should have been, according to our present supposition of a revolving star. And the last column gives the difference between the observed and calculated angles.

Times of the observations.	Observed angles.	Calculated angles.	Difference
Nov 5, 1779 - -	32° 47′	32° 47′	0°
Feb. 23, 1791 - -	22 57	22 11	+ 0 4
Feb. 26, 1792 - -	27 16	21 16	+ 6
Dec. 15. 1795 - -	13 52	17 42	— 3 5
March 26, 1800 - -	18 8	13 41	+ 4 2
April 23, 1800 - -	10 30	13 37	— 3
Dec. 31, 1801 - -	7 58	12 2	— 4
Jan. 10, 1802 - -	10 53	12 1	— 1
Jan. 23, 1802 - -	10 28	11 59	— 1 3
Feb. 28, 1802 - -	13 0	11 53	+ 1
Feb. 11, 1803 - -	7 53	11 0	— 3
March 23, 1803 - -	13 23	10 54	+ 2 2
March 27, 1803 - -	10 53	10 53	0

On looking over the 4th column of this table, it will be found, that the differences between the observed and calculated angles are not greater than may be expected, considering that most of the early measures are single, and cannot have the accuracy which may be obtained by repetition. Even as they are, we must acknowledge them sufficient to ascertain the gradual change in the angle of position of the two stars. In one place, the difference amounts to six degrees; but it will soon appear, that a more accurate annual motion gives a calculated position which takes off much of the error of this measure.

In a conversation with my highly esteemed friend the Astronomer Royal, he happened some time ago accidentally to mention, that Dr. Bradley had formerly observed the two stars of α Geminorum to stand in the same direction with Castor and Pollux. It occurred to me immediately, that if the time of this observation could be nearly ascertained, it would be of the greatest importance to the subject at present under consideration. For, should Dr. Bradley's position be very different from a calculated one, it would induce us at once to give up the idea of a revolving star. The observation was made by Dr. Bradley with a view to see whether any change could be perceived in the course of the year, by which the annual parallax of the stars might be discovered. Dr. Maskelyne, who had this information from Dr. Bradley in conversation, had made a memorandum of it in his papers. He has been so kind as to look for it; and, as soon as he found the note, he sent me the following copy, which I have his permission to transcribe.

" *Double star Castor. No change of position in the two Stars:*
" *the line joining them, at all times of the year, parallel to the line*

"*joining Castor and Pollux in the heavens, seen by the naked* "*eye.*"

Dr. MASKELYNE informs me, that the observation must have been made about the year 1759; and also mentions, that he himself verified the fact, as to the line joining the two stars appearing through the telescope parallel to the line joining Castor and Pollux, in 1760 or 1761; but that he did not examine it at various times of the year.

The advantage of having an angle of position observed in 1759 by Dr. BRADLEY, and so soon after verified by Dr. MASKELYNE, will give us an addition of 20 years to our period. On calculating the right ascension and polar distance of Castor and Pollux for November 5, 1759, it appears, that a line drawn from Pollux through Castor, must have made an angle of 56° 32′ north preceding with the parallel of that star; and, this being also the position of our double star, we have an interval of 43 years and 142 days, for a change of 45° 39′, from the time of Dr. BRADLEY's observation to that of my last measure of the angle. By this we are now enabled to correct our former calculation, which was founded upon a supposition that the first angle of position I had taken was perfect; but this could hardly be expected, and on examination it appears that the measure was 2° 40′ too little. The annual motion, by our increased period, is 1° 3′,1; and the computation of the angles of position in the 3d column of the following Table, as well as the differences contained in the 4th, are made according to this motion.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 5, 1759 - -	56° 32′	56° 32′	0° 0′
Nov. 5, 1779 - -	32 47	35 29	− 2 42
Feb. 23, 1791 - - -	22 57	23 36	− 0 39
Feb. 26, 1792 - -	27 16	22 32	+ 4 44
Dec. 15, 1795 - -	13 52	18 32	− 4 40
March 26, 1800 - -	18 8	14 3	+ 4 5
April 23, 1800 - - -	10 30	13 58	− 3 28
Dec. 31, 1801 - -	7 58	12 12	− 4 14
Jan 10, 1802 - - -	10 53	12 10	− 1 17
Jan. 23, 1802 - -	10 28	12 7	− 1 39
Feb. 28, 1802 - - -	13 0	12 1	+ 0 59
Feb. 11, 1803 - -	7 53	11 1	− 3 8
March 23, 1803 - - -	13 23	10 54	+ 2 29
March 27, 1803 - -	10 53	10 53	0 0

When the result of this Table is compared with that of the former, it will be seen that my observations agree not only very well with Dr. Bradley's position, but even give more equally divided differences than before, so that the excess and differences counteract each other better than in the first Table.

The time of a periodical revolution may now be calculated from the arch of 45° 39′, which has been described in 43 years and 142 days. The regularity of the motion gives us great reason to conclude, that the orbit in which the small star moves about Castor, or rather, the orbits in which they both move round their common centre of gravity, are nearly circular, and at right angles to the line in which we see them. If this should be nearly true, it follows, that the time of a whole apparent

revolution of the small star round Castor, will be about 342 years and two months.

γ Leonis.

Our foregoing discussions will greatly abridge the arguments which may be used, to show that this star and its small companion are also probably united in forming a binary system. But, in order to give more clearness to our disquisition, we shall follow the arrangement which has been used with α Geminorum, and prefix the same letters to our paragraphs. Then, if any one article should appear to be not sufficiently explained, we need but turn back to our first double star, where the same letter will point out what has already been said more at large on the subject; and an application of it may easily be made.

The distance of the stars γ and x, as I shall again call the small one, has undergone a visible alteration in the last 21 years. The result of a great number of observations on the vacancy between the two stars, made with the magnifying powers of 278, 460, 657, 840, 932, 1504, 2010, 2589, 3168, 4294, 5489, and 6652, is, that with the standard power and aperture of the 7-feet telescope, the interval in 1782 was $\frac{1}{4}$ of a diameter of the small star, and is now $\frac{3}{4}$. With the same telescope, and a power of 2010, it was formerly $\frac{1}{2}$ of a diameter of the small star, and is now full 1 diameter. In the years 1795, 1796, and 1798, the interval was found to have gradually increased; and all observations conspire to prove, that the stars are now $\frac{1}{2}$ a diameter of the small one farther asunder than they were formerly. The proportion of the diameter of γ to that of x, I have, by many observations, estimated as 5 to 4.

The first measured angle in 1782, is 7° 37′ north following;*

* In my second Catalogue of double Stars, (Phil. Trans. for 1785, page 48,) the

and the last, which has been lately taken, is 6° 21′ south following. The sum of these angles gives 13° 58′, for the change that has taken place in 21 years and 38 days. To account for this, we are to have recourse, as before, to the various motions of the three bodies.

Single Motions.

(*a*) The motion of x alone cannot be admitted, since it is known that γ Leonis is not at rest. The annual proper motion of this star, according to M. DE LA LANDE, is + 0″,38 in right ascension, and 0″,04 in declination towards the south.

(*b*) γ cannot be the only moving body; because its motion in right ascension only, which, in 21,1 years, at the parallel of γ, amounts to 7″,49, would have long ago taken it away from the small star.

(*c, d, e,*) The sun cannot be the only moving body; because its motion in right ascension will not account for that of γ Leonis, which star therefore cannot be at rest. And, if we were willing to give up the former assumed solar motion, in order to fix upon such a one as would explain the motion of γ, we should be under a necessity to contradict the united evidence of the proper motions of many principal stars which are in opposition to it.

Double Motions.

(*f*) When two motions are proposed, we cannot fix upon γ and x for the moving bodies, unless we should set aside the solar motion, and this, we know, cannot properly admit of a doubt.

angle of position is 5° 24′. This was taken April 18, 1783; and, not being acquainted with the motion of the small star, I supposed it to be more accurate than the former measure.

(g) That we cannot allow O and x to be the two bodies in motion, follows from the insufficiency of the solar motion to account for that of γ, which must be real, or at least partly so.

(h) If O and γ are the moving bodies, the given situations of x, in the years 1782 and 1783, point out an apparent motion of x, which must be intirely owing to the solar parallax; and, therefore, those who will admit this hypothesis, must grant the discovery of the motion of the solar system, and of the proportional parallax of the two stars γ and x. Let us however examine whether any motion of the sun, such as we can admit, will account for the change of position and distance pointed out by my observations of the small star near γ Leonis.

The joint effect of proper motion and parallax, has carried γ from its situation in 1782 to that where we now find it. The small star, having all this time, in appearance, accompanied γ, must have gone through a space of 7",98, in a direction which makes an angle of 8° 30′ south following with the parallel of γ, in order to be at its present distance from it, and at the same time to have undergone the required change of its angle of position. Now, as the supposition we are examining requires this small star to be actually at rest, it will be necessary to assign to the sun an opposite motion of the same velocity, in order to make that of x only an apparent one. The consequence of this will be a retrograde motion of the sun, which it is well known cannot be admitted.

Motion of the three Bodies.

(i) A motion of all the three bodies, is the only way left to explain the phenomena of our double star; and I shall now again point out the very particular circumstances which it is

requisite should all happen together, to produce the intended effect.

Let the motion of the sun, with the same annual velocity 1, as in the case of α Geminorum, be directed towards λ Herculis. Then the effect of this motion will show itself at the place of γ Leonis, in the annual velocity of ,3314, and in a direction which makes an angle of 31° 11′ south preceding with the parallel of that star. In this calculation, I have admitted the distance of the largest of the two stars of γ from the sun to be 3, that of α Geminorum being 2. But, if any other distance should hereafter be considered as more probable, the calculation may be easily adapted to it. The consequence of the parallax thus produced on γ Leonis in 21,1 years, will be an apparent motion of 2″,788 south preceding, in the abovementioned direction; and, on x, it will be in the same time, and in the same direction, 1″,091. As the small star must not be too near γ, we have, in the calculation, supposed it to be at the distance of 4 from O.

The real annual proper motion of γ is required to be 3,5202; and its direction must make an angle of 3° 40′ north following with the parallel. By this motion alone, γ would have passed over a space of 9″87 in 21,1 years; but, when it is combined with the apparent motion arising from parallax, the star will come into its present situation.

The real annual motion of x must be 4,6294, in a direction 0° 20′ south following. This will carry it over 9″,74, in 21,1 years; and, when combined with the apparent motion which the solar parallax will occasion, both together will bring it to its proper distance from γ Leonis, and to a situation which will agree with the last observed angle of position.

From what has been said, it is again evident, that not only as

many particular circumstances must concur in explaining the phenomena of γ Leonis as we have pointed out with α Geminorum, but that a very marked condition is added in our second double star, which requires an adjustment of velocities in γ and x, which shall also fit the same solar motion that was used in α Geminorum. And this proves, that every additional double star which requires the same condition in order to have its appearances explained, will inforce the arguments which have been used, in a compound ratio.

If, on the other hand, we have recourse to the simplicity of the known effects of attraction, and admit the two stars of our present double star to be united in one system, all the foregoing difficulties of accounting for the observed phenomena will vanish. Whatever may be the proper motion of the sun, the parallax arising from that cause will affect both stars equally, on account of their equal distance from the sun. The proper motion of γ Leonis also may be in any direction, and of any given velocity, such as will agree best with astronomical observations; since the motion of a system of bodies will not interfere with the particular motion of the bodies that belong to it, so that our secondary star will continue its revolution round the primary one without disturbance.

It will now be necessary to examine the observed angles of position, and to compare them with calculated ones; but, as there has been a change in the distance of the two stars, it is evident that, if they revolve in circular orbits, the situation of the plane of their revolution must be considerably inclined to the line in which we see the principal star.

Let N F S P, Fig. 2, be the orbit in which x revolves about γ placed in the centre. Suppose a perpendicular to be erected at

γ leading to O, not expressed in the figure. By an observation of Feb. 16, 1782, we have the angle $F \gamma x = 7° 37'$ north following; and the proportion of the apparent diameter of γ to that of x has been given as 5 to 4. It has also been ascertained, that the vacancy between the apparent diameters, when the first angle of position was taken, was $\frac{1}{4}$ diameter of the small star; and the last angle of position being $6° 21'$ south following, with a distance between the stars of $\frac{3}{4}$ diameter of the small star, we obtain the two points or centres of the small stars $x x'$, through which an ellipsis $a b x x' c d$ may be drawn about γ. This will be the apparent orbit in which the small star will be seen to move about γ, by an eye placed at O. And the inclination of the orbit to the line in which we see the double star, will be had sufficiently accurate to enable us to give a calculation of the several angles of position that have been taken. The ellipsis we have delineated shows that the small star, in its first situation x, could not be much past its conjunction at b, and that, consequently, in passing from x to x', the parts of the apparent elliptical arch, which are projections of the real circular arch $b b'$, would be described in times nearly proportional to the time in which the whole arch has been described. Upon these principles, the 3d column of the following Table has been calculated.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Feb. 16, 1782 - -	7° 37′ *nf*	7° 37′	0° 0′
April 18, 1783 - -	5 24 *nf*	6 51	− 1 27
Jan. 24, 1800 - -	3 16 *sf*	4 15	− 0 59
Feb 19, 1800 - -	3 23 *sf*	4 18	− 0 55
March 26, 1800 - -	3 47 *sf*	4 22	− 0 35
Jan. 26, 1802 - -	6 4 *sf*	5 35	+ 0 29
Feb. 10, 1803 - -	3 33 *sf*	6 16	− 2 43
March 22, 1803 - -	6 32 *sf*	6 20	+ 0 12
March 26, 1803 - -	6 21 *sf*	6 21	0 0

The difference between the calculated and observed angles, contained in the 4th column of the preceding Table, is so little, that we may look upon the gradual change of these angles as established by observation; and we may form a calculated estimate of the time which will be taken up by the mutual revolution of the two stars. The apparent places $x\ x'$, being referred to their real ones, give the arch bb', which has been described in 21 years and 38 days; and this arch, seen from the centre γ, is about 6° 20′: it follows, that the length of a whole revolution of our small star round γ Leonis, will be about 1200 years.

ϵ *Bootis.*

This beautiful double star, on account of the different colours of the stars of which it is composed, has much the appearance of a planet and its satellite, both shining with innate but differently coloured light.

There has been a very gradual change in the distance of the two stars; and the result of more than 120 observations, with

different powers, is, that with the standard magnifier, 460, and the aperture of 6,3 inches, the vacancy between the two stars, in the year 1781, was $1\frac{1}{2}$ diameter of the large star, and that it now is $1\frac{3}{4}$. By some earlier observations, the vacancy was found to be considerably less in 1779 and 1780; but the 7-feet mirror then in use was not so perfect as it should have been, for the purpose of such delicate observations. By many estimations of the apparent size of the stars, I have fixed the proportion of the diameter of ε to that of x, as 3 to 2. August 31, 1780, the first angle of position measured 32° 19′ north preceding;* and, March 16, 1803, I found it 44° 52′, also north preceding: the motion, therefore, in 22 years and 207 days, is 12° 33′. It should also be noticed, that while the apparent motion of α Geminorum, and of γ Leonis, is retrograde, that of ε Bootis is direct.

A proper motion in this star, if it has any, is still unknown; our former arguments, therefore, cannot be applied to it, without some additional considerations; and, as many others of my double stars will stand in the same predicament, I shall give an outline of what may be said, to show that this, and probably many of the rest, are also binary systems.

Single Motions.

(*a*—*e*) If ε Bootis is a star in which no proper motion can be perceived, we may infer, from the highly probable motion of the solar system, that this star, which is of the 3d magnitude, and on that account within the reach of parallax, must have a real motion, to keep up with the sun, in order to prevent an

* The angle of position, in my first Catalogue of double Stars, Phil. Trans. for 1782, page 115, is 31° 34′ (it should be 54′) north preceding. This will be found to be a mean of the three first measures hereafter given in a Table of positions.

apparent change of place, which must otherwise have happened. In this case, no single motion can be admitted to explain the phenomena of our double star. But, if a real proper motion of ε Bootis should hereafter be ascertained, the arguments we have used in the case of γ Leonis, will lead to the same conclusion.

Double Motions.

(*f*) ε and *x* cannot be the moving bodies; and our former argument (*f*) will apply to every double star whatsoever.

(*g*) O and *x* cannot be alone in motion; for, if no motion in ε can be perceived, it must move in a similar manner with the sun, and none of the three bodies will be at rest. But, if its proper motion shall hereafter be found out, it must either be exactly the reverse of the solar motion, and therefore only an apparent one, or it will be more or less different. In the latter case, all the three bodies must be in motion; in the former, the exact quantity of the solar motion will be discovered, and the relative parallax of many stars may be had by observation.

(*h*) If O and ε are the two bodies in motion, and if at the same time no motion in ε can be perceived, then the apparent motion of *x* must be intirely owing to the different effect of the solar parallax on ε and *x*; but the effect of the solar parallax on *x*, can only be in a direction contrary to the motion of the sun which, being north following the small star, whether it be nearer or farther from us than ε, must have an apparent motion towards the south preceding part of the heavens. But this is directly in opposition to my observation of the motion of the small star, which, these last 23 years, has been directed toward the north following.

Motion of the three Bodies.

(*i*) Let the motion of the sun be again towards λ Herculis; then, if no motion in ε Bootis be perceivable, it must move exactly like O. Highly improbable as it is, let it be admitted. Then, in addition to this extraordinary supposition, a third motion is also required for x, which, aided by the solar parallax, is to carry it likewise within a quarter of a diameter of ε, into the same place where, though unperceived, the large star has been carried by its own motion; that is, in order to be apparently at rest, the sun, ε Bootis, and its small companion, must all move exactly alike, setting aside the very little difference in the position and distance of the small star, which, in the whole, amounts to little more than 6-tenths of a second; than which, certainly nothing can be more improbable.

But, if ε shall hereafter be found not to have been at rest during the time of my observations upon it, then its place will be given; and, since also the situation of x, with respect to ε, is to be had from my angles of position and distances of the two stars, the case will be similar to that which has already been considered, in the paragraph (*i*), under the head of γ Leonis.

I may here add a remark with regard to ε Bootis, which will be applicable to several more of my double stars. In the milky-way, a multitude of small stars are profusely scattered, and their arrangement is very different from what we perceive in those parts of the heavens which are at a considerable distance from it. About ε Bootis, which is situated in what I have formerly called figuratively a nebulous part of the heavens,* there are, comparatively speaking, hardly any stars; and, that so

* See Phil. Trans. for 1784, page 449.

remarkable a star as ε should have a companion, seems almost to amount to a proof that this very companion is, as it appears to be, a connected star. The *onus probandi*, therefore, ought in justice to fall to the share of those who would deny the truth of what we may call a fact; and I believe the utmost they could do, would be to prove that we may be deceived; but they cannot show that this star has no connection with ε Bootis.

This argument will be much supported, when we consider that many of the double stars in the milky-way are probably such as have one of the scattered stars, nearly in the same line, at a great distance behind them. In this case, the two stars of the double star have no connection with each other; and the great number of them in the milky-way, is itself an indication of this effect of the scattered multitude of small stars. In the single constellation of Orion, for instance, we have no less than 43, pointed out by my catalogues; ten of which are of the first class, and yet have undergone no change of distance or position since I first perceived them. But, with apparently insulated stars, such as ε Bootis, the case is just the reverse.

If, in consequence of our former arguments, and the present remarks, we place ε Bootis among the stars which hold a smaller one in combination, we may delineate its orbit as in Plate VIII. Fig. 3.

Let PNFS represent a circle, projected into the elliptical orbit $axx'bcd$. ε is the large star; and xx' are the first and last measured north preceding situations of the small one, as given in the following Table.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
August 31, 1780 -	32° 19′	33° 58′	− 1° 39′
March 13, 1781 - -	30 21	34 13	− 3 52
May 10, 1781 - -	33 1	34 18	− 1 17
Feb. 17, 1782 - -	38 26	34 40	+ 3 46
August 18, 1796 -	45 32	41 40	+ 3 52
Jan. 28, 1802 - -	49 18	44 19	+ 4 59
August 31, 1802 - -	46 47	44 36	+ 2 11
March 23, 1803 -	43 43	44 52	− 1 9
March 26, 1803 - -	44 52	44 52	0 0

The real motion from *b* to *b′* is projected into that from *x* to *x′*; and, while the elliptical arch subtends an angle of 12° 33′, the circular one will be about 4° 50′.

From the figure of the orbit, we may conclude that the small star, in its first position, at *x* or *b*, was not more than between 30 and 40 years past its conjunction; and that, consequently, the parts of the arch *xx′*, were nearly proportional to the times of their being described. The positions have been calculated upon this principle; but with some allowance for the first observed angle, which I suppose to have been a little too small; and, though the differences of the observed and calculated angles are pretty considerable, the observations are still sufficiently consistent to prove the gradual change of the situation of the small star.

The quantity of the change in 22 years and 207 days, will show that a periodical revolution cannot take up less than 1681 years. The real figure and situation of the orbit, with many other particulars, are still unknown; it is, therefore, unnecessary

to point out the uncertainties in which the investigation of the periodical time of the small star about ε Bootis must long remain involved.

ζ *Herculis.*

My observations of this star furnish us with a phenomenon which is new in astronomy; it is, the occultation of one star by another. This epoch, whatever be the cause of it, will be equally remarkable, whether owing to solar parallax, proper motion, or motion in an orbit whose plane is nearly coincident with the visual ray. My first view of this star, as being double, was July 18, 1782. With 460, the stars were then $\frac{1}{2}$ diameter of the small star asunder. The large star is of a beautiful bluish white; and the small one ash-coloured.

July 21, of the same year, I measured the angle of position, 20° 42′ north following. With the standard power, the distance of the stars remained as before. With 987, they were one full diameter of the small one asunder.

In the year 1795, I found it difficult to perceive the small star; however, in October of the same year, I saw it plainly double, with 460; and its position was north following.

Other business prevented my attending to this star till the year 1802, when I could no longer perceive the small star. Sometimes, however, I suspected it to be still partly visible; and, in September of the same year, with 460, the night being very clear, the apparent disk of ζ Herculis seemed to be a little lengthened one way. With the 10-feet telescope, and a power of 600, I saw the two stars of η Coronæ very distinctly; and, having in this manner proved the instrument to act well, I directed it to ζ Herculis, and found it to have the appearance of a

lengthened, or rather wedge-formed star; after which, I took a measure of the position of the wedge.

Our temperature is seldom uniform enough to permit the use of very high powers; however, on the 11th of April, 1803, I examined the apparent disk, with a magnifier of 2140, and found it, as before, a little distorted; but there could not be more than about $\frac{3}{8}$ of the apparent diameter of the small star wanting to a complete occultation. Most probably, the path of the motion is not quite central; if so, the disk will remain a little distorted, during the whole time of the conjunction. Our present observations cannot determine which of the stars is at the greatest distance; but this will occasion no difference in the appearance; for, if the small star should be the nearest, its light will be equally lost in the brightness of the large one.

The observations I have made on this star, are not sufficient to direct us in the investigation of the nature of the motion by which this change is occasioned.

We may however be certain, that with regard to

Single Motions,

(a, b) Neither x nor ζ can be supposed to be the only moving bodies, without contradicting the highly probable arguments for the sun's motion.

(c, d) If we admit the sun to be the moving body, the stars ζ and x being at rest, we may calculate the effect of the solar parallax upon them, as follows. Let O move towards λ Herculis, with the annual velocity 1, as in the case of α Geminorum; then, from the situation and magnitude of the large star of ζ Herculis, which we will suppose 4m, the effect of the solar motion at ζ will be only ,0522; and, at x, supposed to be at the distance 5m,

t will be ,0418. This will show itself at the parallel of ζ in a lirection of 25° 5′ north preceding, the solar motion being in he opposite direction south following. But this parallax will only produce, in 20 years and 10 months, an apparent change of 0″,444 in ζ, and of 0″,355 in x; and will separate the stars, instead of bringing them to a conjunction.

(*e*) A considerable advantage may be gained, by placing x at a little more than $\frac{1}{3}$ the distance of ζ from O. For as, in the abovementioned time, this would make the effect of parallax upon it 1″,18, a conjunction should now take place. But then the stars, though very near each other, would not be quite in contact; much less could one of them occasion an occultation of the other. The supposition also, that the small star should be only $\frac{1}{3}$ of the distance of the large one from us, is not very favourable to the hypothesis.

δ Serpentis.

This double star has undergone a very considerable change in the angle of position, but none in the distance of the two stars. The 5th of September, 1782, an accurate measure of the position was 42° 48′ south preceding; and February 7, 1802, it measured 61° 27′ south preceding. In 19 years and 155 days, therefore, the small star has moved, in a retrograde order, over an arch of 18° 39′.

Every argument, to examine the cause of this motion, which has been used with ε Bootis, in the paragraphs from (*a*) to (*ι*), will completely apply to this star; from this we may conclude, that the most natural way of accounting for the observed changes, is to admit the two stars to form a binary system. In this case we calculate, with considerable probability, that the periodical

time of a revolution of the small star round δ Serpentis, must be about 375 years.

γ Virginis.

This double star, which has long been known to astronomers,* has undergone a visible change since the year 1780, when I first began my observations of it. The 21st of November, 1781, I measured the position of the two stars, which was 40° 44′ south following. The stars are so nearly equal, that I have but lately ascertained the following one to be rather larger than its companion; the position, therefore, ought now to be called north preceding. By a mean of three measures, that were taken on the 15th of April, 1803, the angle was 30° 20′ *np*.

The distance, as far as estimations by the diameter can determine, when the stars are so far asunder as these are, remains without alteration. May 21, 1781, they were 2½ diameters asunder; and, by estimations lately made, with the same instrument and power as were used 21 years ago, the stars are still at the same distance of 2½ diameters.

A very small proper motion in declination, of 0″,02 towards the south, has been assigned to this double star;† but the quantity is hardly sufficient for us to rely much upon the accuracy of the determination. I shall therefore rather consider γ Virginis as one of the stars of which we have no proper motion ascertained; and the arguments to which I shall refer, will consequently be those which have been given with ε Bootis.

The change of the angle of position, in the time of 21 years and 145 days, amounts to 10° 24′; from which we obtain the

* *Memoires de l'Academie des Sciences.* Ann. 1720.

† *Connoissance des Temps,* Année VI. page 213.

annual motion of 29',16. The observed and calculated angles, with their differences, on which it will not be necessary to make any remarks, are in the following Table.

Times of the observations.	Observed angles.	Calculated angles.	Differences.
Nov. 21, 1781 - -	40° 44'	40° 44'	0° 0'
Jan. 29, 1802 - -	28 22	30 51	− 2 29
April 15, 1803 - -	30 20	30 20	0 0
May 28, 1803 - -	32 2	30 17	+ 1 45

As a confirmation of the accuracy of these observations, we may have recourse to a position of the same stars, deduced from the places of them, as they are given in MAYER's Zodiacal Catalogue. By two observations, reduced to the beginning of the year 1756, the preceding one was 3'',8 before the other in right ascension, and 5'',3 more north than that star. From this we calculate the position, which was 54° 21' 37'' north preceding. The interval from the 1st of January, 1756, to the 21st of November, 1781, is 25 years and 325 days. When this is added to the period I have given, we have 47 years and 105 days, for a motion of 24° 2'. The annual motion, deduced from this lengthened period, which is 30',5, differs less than 1½ minute from that which has been calculated from my observations. With the assistance, therefore, of MAYER's observation, which greatly supports our calculation, we may conclude, that the two stars of γ Virginis revolve round each other in about 708 years.

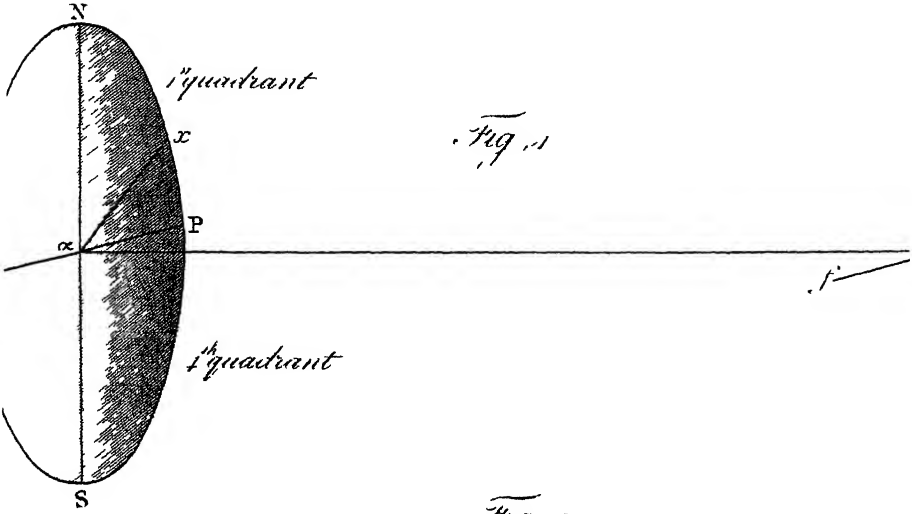


Fig. 1

Fig. 2

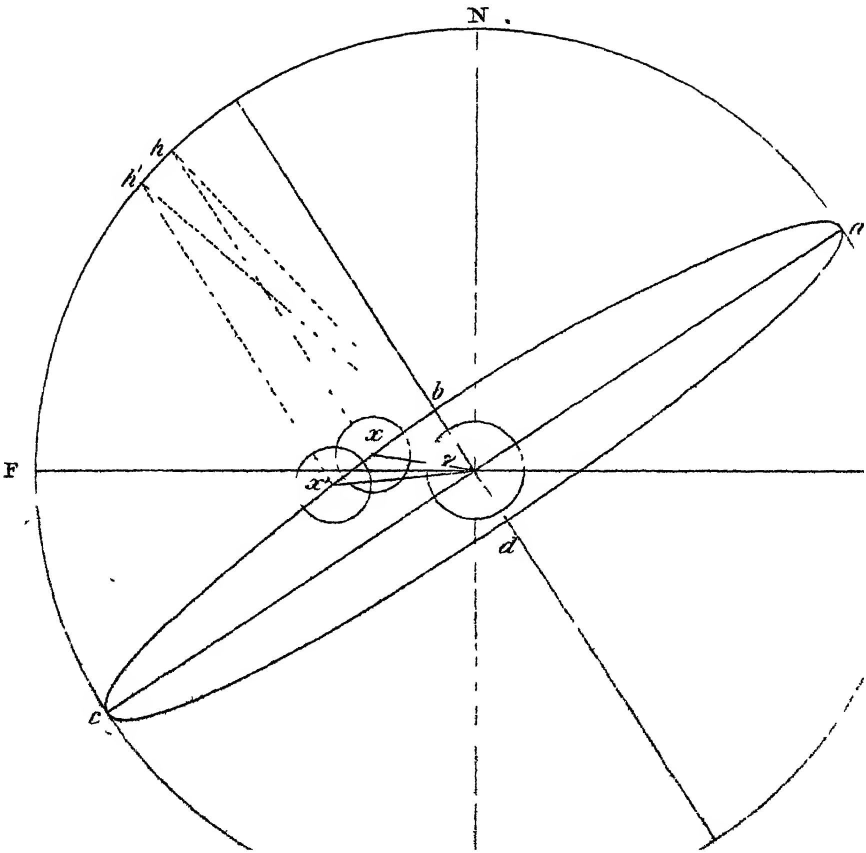
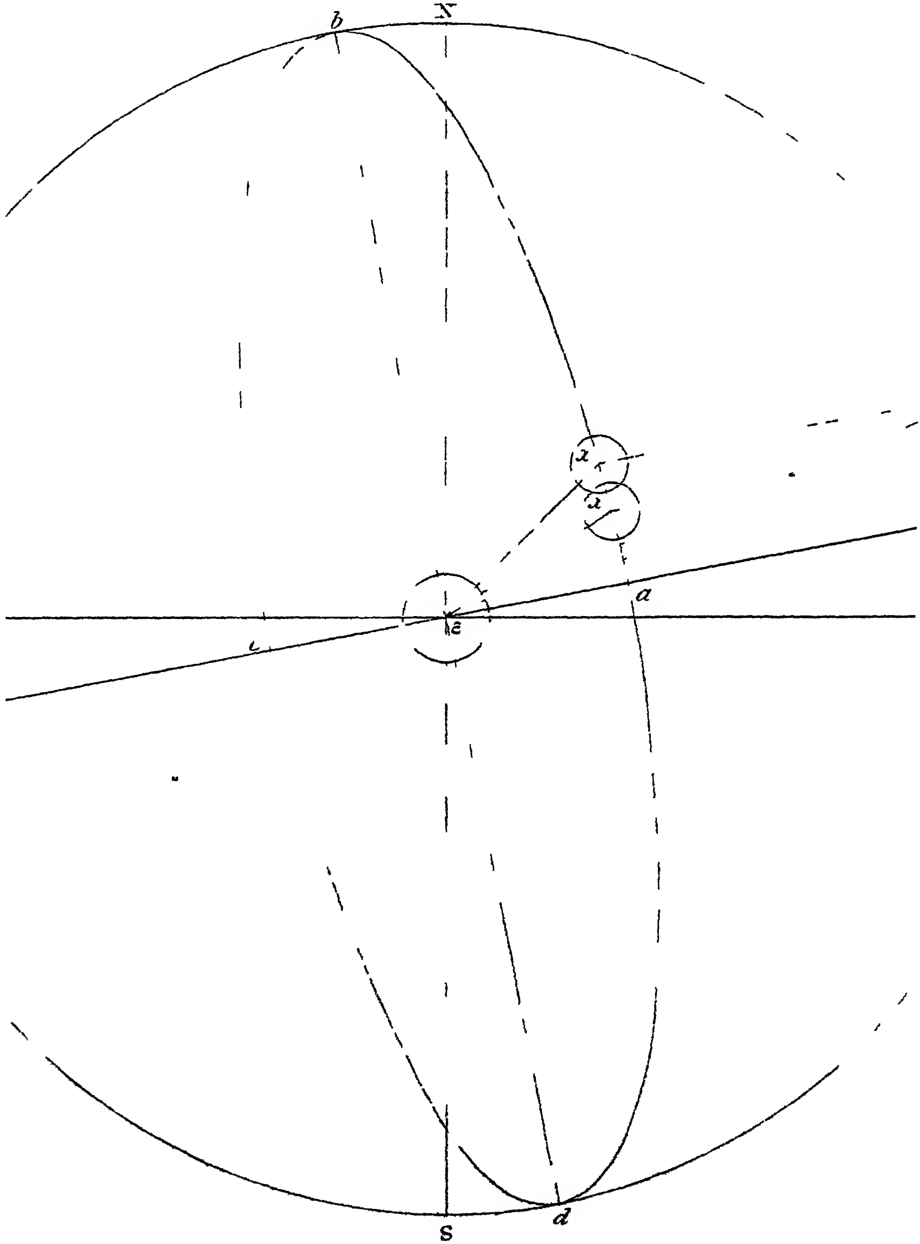


Fig 3



XVI. *An Account of the Measurement of an Arc of the Meridian, extending from Dunnose, in the Isle of Wight, Latitude 50° 37′ 8″, to Clifton, in Yorkshire, Latitude 53° 27′ 31″, in course of the Operations carried on for the Trigonometrical Survey of England, in the Years* 1800, 1801, *and* 1802. *By Major* William Mudge, *of the Royal Artillery, F. R. S.*

Read June 23, 1803.

SECTION FIRST.

In the last account presented to the Royal Society, I expressed my intentions of making the operations which were to follow that period, subservient to the purpose of measuring a portion of the meridional arc, running from Dunnose, in the Isle of Wight, into the northern part of Yorkshire. In the account referred to, (See Phil. Trans. for 1800, page 565,) I stated my reasons for adopting that resolution, and my hopes that Mr. Ramsden would shortly finish the zenith sector which his Grace the Duke of Richmond had bespoken of him, when Master General of the Ordnance. As that celebrated artist, from the beginning of the year 1800 till the middle of the following summer, had proceeded with little interruption, except from illness, towards its completion, the whole was brought so near to a conclusion before he died, that Mr. Berge found no difficulty in rendering it sufficiently perfect.

It is proper I should state, more fully than I have formerly

lone, my reasons for selecting Dunnose as one of the extremities of my meridional line, and also those for preferring its meridian to any other; which I shall do as briefly as possible.

In a country whose surface, throughout its whole extent, is equally diversified with hilly ground, that particular part of it should be chosen, for carrying on a meridional measurement, which comprehends the most extensive arc. This arises from the necessary consequence which attends an operation in a country so circumstanced; as, possibly, no spot fixed on for a place of observation, could be supposed free from the effects of the unequal attraction in the adjoining matter. In such a country, therefore, a measurement upon the most extensive arc, must give the most accurate conclusion; for the errors arising from the cause here mentioned, like those of observation, lessen in their effects, on their application to arcs of increasing magnitude.

If Great Britain were a country thus diversified, the most eligible part would be that where the meridian from Lyme, in Dorsetshire, passes northward into Scotland. The difference of latitude between that place and Aberdeen, near to which that line cuts its parallel, is 4° 47′, nearly. But, however great the advantages attending such a length of arc might be, under the general circumstances of accurate terrestrial measurement, and accurate observations at its extremities, no beneficial consequences could be expected to attend the placing of the sector at intermediate stations; as the arc would be found running, almost every where, through a country abounding with hills, considerable both in magnitude and number.

Under this consideration, I determined to measure a portion of the meridian which proceeds from Dunnose to the mouth of the Tees; because, from inquiry, I had reason to suppose it the

longest meridional arc in Britain, free from any apparent obstruction. And I was led to select Dunnose for one of its extremities, as observations made there, in conjunction with others at Greenwich, would enable me to make corrections of the latitudes of places given in our former papers, if found necessary. By fixing on Dunnose, I had also the means of ascertaining the distance of the Royal Observatory from the northern or southern end of my line, and, consequently, of connecting it with the parallels of Dunkirk and Paris.

Dunnose being fixed on, my subsequent endeavours were directed towards carrying on the triangles, as nearly as I possibly could, in the direction of its meridian, selecting the stations so that their sides might be properly inclined to it, and of sufficient length. In choosing the station at the northern extremity, I was careful to select it as near the meridian of the southern one as possible, and likewise in the neighbourhood of some open spot of ground, proper for the measurement of a base of verification. A station having these advantages, was found near Clifton, a small village in the vicinity of Doncaster; and a level of sufficient extent for a base, on Misterton Carr, in the northern part of Lincolnshire.

In the composition of this account, I wish to confine myself to that part of my operations which relates merely to the matter expressed under its title. I am possessed of materials sufficient for another Paper; and shall give about thirteen hundred triangles, principal and secondary, when next I present an account of the Survey to the Society: professing this, I shall now say, that in 1800 and 1801, the angles of the triangles constituting the meridional series were observed; and that, in the latter year, the new base was measured on the abovementioned

Carr. I should not omit mentioning in this article, that while the instrument was at Clifton, the direction of the meridian was obtained from numerous observations on the pole star, at the times of its greatest eastern and western elongations from the meridian. It will be recollected, that similar observations were made at the station on Dunnose, in 1793; (see Phil. Trans. for 1795, page 460;) so that nothing relating to the terrestrial part of the operation remained to be performed at the expiration of 1801.

On my arrival in town, after the measurement of the base of verification in the north, I had the happiness of finding the zenith sector nearly completed. Little remained to be done, besides the dividing of its arch; an operation which Mr. Berge proposed to defer till the following spring: it was then divided, and the instrument, being otherwise complete, was delivered into my hands in April. An observatory of convenient form having been previously made, the sector was immediately erected in the Tower; and, from thence, with the permission of Dr. Maskelyne, it was sent to the Royal Observatory at Greenwich.

It is now necessary I should enter into a minute detail of this instrument's construction, giving a description of its several parts, with references to proper drawings. If, indeed, I had no other motives, I should perhaps be induced to do it from justice to the merit and memory of the ingenious inventor; who seems to have exerted his talents to the full extent of the hopes he entertained, of rendering this instrument the first of its kind.

General Description of the Zenith Sector.

In the sector I am going to describe, Mr. Ramsden has obviated the inconveniences attendant on the use of former sectors;

and has also diminished, in a very considerable degree, the errors unavoidably resulting from their imperfect construction. The principles on which he has founded the several improvements, consist in the means of uniting the sectorial tube to its axis, so as to ensure the permanency of the length of its radius, when erected for observation; more accurate methods of adjusting the instrument vertically; and an easy way of placing the face of its arch in the plane of the meridian. Another circumstance of moment was, some contrivance by which the plumb-line should be brought precisely over the point, marking the centre of the circle of which the divided arch of the sector should be a part. The last desideratum, the ingenious artist procured, by applying the same contrivance which so eminently displayed his skill, in the construction of the quadrant belonging to his Grace the Duke of MARLBOROUGH; a contrivance by which the plumb-line can be as readily adjusted over the required point, by a person standing on the ground, as any adjustment, or other act within his reach, can be performed. A description of this, as well as of the means by which the instrument is rendered vertical, and otherwise correctly prepared for observation, will be given, with the assistance of plates.

Plate IX. Exhibits a general view of the sector erected for observation: it consists of two parts; 1st, the frame which supports the apparatus to which the sectorial tube is attached; 2d, the work constituting that apparatus, with the tube itself.

The external frame or stand is made of mahogany, and unites strength with simplicity of construction. In shape, it is an obtruncated pyramid, whose base is a square of six feet in length, and whose vertex is half of it. This frame, although light in its make, is yet, when united by means of square-headed screws,

sufficiently firm. Inside this hollow stand is erected another frame, of the same substance, strong and well made, within which is suspended the sector; its frame being supported at top in every lateral direction, and sustained at bottom by a cone resting in a metallic concavity, the figure of which may be imagined, by supposing an arch of a circle to revolve round a tangent to one of its extremities. A cylinder, in the upper part of the interior stand, finds its place in an opening of an octagonal shape in the exterior frame, and, by a simple contrivance, is retained in that situation, while the sector and apparatus revolves on the cone. Thus, a ready means presents itself of turning the instrument round, with the face of its divided arch towards the east or west. It may be steadily retained in any position, by clamping it to the brass work of an azimuth circle, attached to the bottom of the external frame.

The direction of the meridian, at the place of observation, having been previously obtained from double azimuths of the pole star, this instrument admits of being placed in that direction very accurately. A telescope, twenty-nine inches in length, is attached to the side of the great tube, or rather, may be occasionally placed on a frame permanently fastened to it, having its axis in the plane of the divided arch, and very nearly at right angles to its radius. On the divided azimuth circle below, the angular bearing of any proper object may be set off, by turning the sector round till that object bisects the cross wires in the little telescope, and then noting the vernier. If the axis of the sector be horizontal, and the interior frame set perfectly upright, the instrument may be turned round from one point of the compass to the other, and properly adjusted for observation, in a few minutes.

In this general description, I am now to speak of one of the most ingenious contrivances attending the sector, which is, the means of readily adjusting the plumb-line in its several positions. I refer to the Plates and their descriptions, for a full account of it; but, as it will enable the reader to understand that which represents the instrument in its perspective view, (Plate IX.) I shall shortly describe this part.

The telescope of the sector is nearly eight feet long, and has an object-glass of four inches in diameter. It is attached to an axis, similar in shape to that of a transit-instrument, having at one end a lens, and near to the tube an arrangement of brass work, carrying a thin and diaphanous slice of mother-of-pearl, having, as appears to the naked eye, a dot upon it. The centre of this dot is by construction the true centre of the conical axis, and consequently of the circle of which the divided limb is a part. It is unnecessary to say, in this place, how that diaphragm was so adjusted as to have the centre of its dot where it should be, or the means by which it has been permanently fixed; it suffices that I say the point was placed most accurately, and the diaphragm fastened so firmly in the cone, that no readjustment of this part has been found necessary, since the sector came into my hands.

As the axis is hollow, a light, as that of a candle, held at its open end, is transmitted through the mother-of-pearl, which, stopping a part of its rays, exhibits a circle of red light to an eye looking through the lens at the opposite end of the axis; a well defined and exceedingly small dot appearing in the middle of the illuminated circle. Through proper openings in the upper and under parts of the axis, and suspended from a point not connected with it, passes the plumb-line, having its

position by construction *close to the dot;* so that, by looking through the axis in this way, the plumb-line appears like a small black line on the face of the mother-of-pearl.

Now it is evident that, to an eye thus placed, when the instrument is adjusted for observation, the plumb-line should appear as if accurately bisecting the dot. To give, therefore, the observer the means of moving it to the right or left, when standing on the ground, (avoiding thereby the inconvenient necessity of elevating himself on steps as high as the axis,) Mr. RAMSDEN placed a microscope, about 5 feet in length, parallel to the telescope, on the outside of the interior mahogany frame. This microscope, bent as it were at right angles at both ends, has one of them open, and placed close to the pivot of the axis carrying the small lens. In the upper part of the microscope, and just under its roof, is placed a speculum, inclined, at an angle of 45°, to the line passing through the centre of the sector's axis, and close to its end. This reflector receives the converged images of the dot and wire on the illuminated spectrum, and transmits them down the tube of the long microscope: the rays, falling on a *plano-convex* glass, at no great distance from the bottom, are finally sent out to the eye by a prismatic glass at the end of the tube. Thus viewed, that which to the naked eye above appeared a small dot on the illuminated lamina, when magnified, as delivered below, is seen to be a small and well defined circle with a luminous area, admitting of the most accurate means of deciding on the right position of the plumb-line, by exhibiting small portions of light between it and the periphery of the little circle.

The mode of illuminating the hollow axis is likewise ingenious. On the side of the interior mahogany frame, and opposite

to the vertical microscope, is suspended a lamp on two arches. At the back of it is a hollow cylindrical recess, in which is placed a polished metallic segment of a sphere. This concave reflector is attached to the cylinder, by means which give it any position required; so that the image of the burning wick, in the hollow of the lamp, may be thrown at pleasure on any spot above.

From the end of the conical axis, on the same side with this lamp, projects a small brass arm, carrying at its extremity a speculum, whose surface is placed at 45° with the vertical, and directly opposite the open end of the sector's axis. When the image of the burning wick is thrown from the concave reflector on the flat one above, the light passes through the hollow axis, illuminating the mother-of-pearl, and is, at last, sent down the microscopic tube to the eye below. This contrivance, collectively taken, is *unique*, and is full as accurate in its operation as ingenious in itself. From its nature, granting perfection of work, there can be no parallax between the dot and the wire. The images of the illuminated circle and wire, (plumb-line,) are coincident on the upper surface of the prismatic eye-glass, and transmitted so. In short, the whole has been so well managed, that the plumb-line can be made to bisect the dot or little circle, as accurately as the points on the divided limb of the sector. I consider this general description of this part of the instrument sufficient for the present; the proper plate, and its appropriate explanation, will supply what yet remains to be said.

The plumb-line is suspended above the upper part of the axis, from a point connected with the extremity of a bent lever, moveable round its fulcrum. The other end of the lever is acted on by a helical spring, which presses downwards, and causes

it to bear against a screw passing through a head of metal beneath that end. The extremity of the long screw is square, and has its place in a pipe attached to a mahogany rod, divided in the middle by a universal joint. One extremity of this rod is brought down, and received in a socket within convenient reach of the observer, who, looking at the image of the dot and wire, turns the rod, thus connected with the bent lever, and thereby gives motion to the plumb-line.

The pivots of the sector's axis are of bell metal, ground perfectly true and smooth. They rest in Ys, firmly attached to the upper part of the frame. The method of uniting the plates carrying those Ys, is as follows: at the upper part of the mahogany frame are four hollow strong cylinders of brass, which pass through the wooden work, and, at the same time, serve to connect very firmly the two sides of the upper part of the frame. These cylinders project about six inches beyond the surface of the wood, and have screws and nuts at their ends.

The brass plates furnished with the Ys, have four holes in each of them, which answer to the ends of the screws, and are attached to the cylinders furnished with those screws, by the respective nuts. In the Ys, the pivots of the axis are placed; and, as a means of adjusting each Y is fixed to each plate, any position, within a certain limit, may be assigned to the sector and its axis.

To prevent the pivots of the axis from moving to and fro in a sidelong direction, Mr. Ramsden adopted a contrivance for keeping them, at all times, in the same constant position in the Ys. This *desideratum* was not to be dispensed with; for, if the ends of the axis, from the thickening of the oil, or the accumulation of dust, should work laterally in their angles, the distance

between the plumb-line and arch would be continually varying; a perplexing evil, and the cause of great inaccuracy. The sum of this contrivance consists, first, in one of the Y plates having a small piece of brass screwed flat upon it, with a roller or friction-wheel at its end, which reaches just high enough to meet the vertical surface of the pivot a small distance within its circumference; and, secondly, in the other Y plate having a small apparatus, consisting of a lever furnished with another friction wheel and a spring, at its other extremity. This last mentioned roller, from the spring's action, presses against its proper pivot, and thrusts the other against the fixed wheel. By these simple but ingenious means, the axis is always retained in its proper situation with respect to the Ys.

To prevent the axis from bending, by the preponderance of the telescope and arches, Mr. Ramsden added braces and counterpoising weights. The braces are four in number, each being a hollow tube: they are fastened both to the axis and the telescope. Their principal uses consist in obviating the possibility of the telescope bending from accidental pressure, or vibrating when lightly touched. The method of preventing the telescope from sinking, or, in other words, the axis from bending, is by the use of levers and the abovementioned weights. These levers, two in number, are attached to the interior mahogany frame at top, the fulcrum of each being immoveable. At the end of each lever farthest from the tube, a weight is suspended, from a hook capable of being placed nearer to, or farther from, the fulcrum, at pleasure; thereby affording the means of raising the other end of the lever up against the cone, with any required degree of force. That extremity of the lever, so pressing upwards, has two large friction-wheels, which apply

themselves to the sides of the conical axis, but do not retard the free motion of it, when the telescope is moved in the direction of the plane of its arch. These wheels, two on each lever, support the axis near the junction of the telescopic tube; and, as a few ounces only are by these means suffered to press on the pivots, no bending takes place in the cones.

From the middle of the two uppermost horizontal cylinders, which unite the sides of the interior frame at top, and which receive, with the two beneath them, the respective Y plates, arises a small but substantial apparatus of metal, embracing a hollow brass cylinder, of five inches in diameter, and about three deep, which passes up into an opening in the upper part of the external mahogany frame. This cylinder, with its corresponding stand, are sustained, without any sort of shake, by a helical spring. This mode, with that of supporting the azimuth circle below, are so well managed, that when the instrument is properly adjusted for observation, the axis of the sector continues perfectly horizontal, in every position of the frame.

There is likewise a very convenient method of sustaining the sectorial tube in any required position for observation. Across the interior frame, about the height of the graduated arch, run two long brass axles, with two wheels on each, one precisely in the middle of the axle, (and consequently in the same plane with the line vertically cutting the middle of the telescope,) and the other close to the pinion at the end of the axle. From a steel pin, something peculiar in its construction, situated near the end of the telescope, proceeds a string, which is wound eight or ten turns round the pulley. Attached to the inside of the interior frame, and just above the wheels nearest to the end of the axle, is another pulley, over which, passing into a long and

narrow wooden compartment, is thrown a string, having a hook and a proper apparatus for receiving the moveable weights. The other end of this string is fastened to the pulley close to the axle, and gives motion to the telescope, or retains it in equilibrio, according to the arrangement of the two sets of weights, which consist of fifteen pieces of brass. By these means, all injurious pressure is taken off the point of the micrometer-screw, against which the telescope may be made to bear, with any required degree of force.

To cause the string passing over the middle of the pulley to draw in the exact direction of the limb's plane, Mr. Ramsden placed four small friction-wheels close to the eye end of the telescope, two on each side, and between each pair of wheels a steel pin, made like a T, with a hook at the end, to receive a string. This pin, where it applies to the wheels, is something in shape like a double cone, and is passed behind them. It always, from its construction, assumes the same position with regard to the friction-wheels; from which circumstance, the sustaining string is ever found in the plane passing through the centre of the telescope and the middle of the pulley.

The micrometer-screw, for measuring minutes and seconds, performs its operations in the usual way: it is moved backwards or forwards on a brass arch, parallel to the limb of the sector, and placed against the mahogany frame behind. To this arch the apparatus carrying the micrometer-screw is clamped; and it is adjusted, or brought parallel to the limb, by screws, so that the point of the micrometer-screw always bears exactly on the same part of the polished steel head, at the end of the sector.

The principal wires in the focus of the eye-glass are two, and are at right angles to each other. There are, indeed, two others

parallel to the meridional one, and at equal distances from it; they were placed there with a view of rendering an adjustment of the horizontal wire sufficiently easy. These are illuminated by means of the lamp which carries the concave reflector before spoken of. There is a hole, with a lens, in the side of the telescope, directly opposite to the lamp, having behind it a diaphragm of brass, coated with plaister of Paris, and inclined to the vertical axis of the tube, at an angle of 45 degrees. The quantity of light, suited to the circumstances of the observation, is regulated by coloured glasses, placed over the hole in the side of the tube.

The plummet, suspended at the wire, falls into a cylindrical cup, swinging by two pins on its edge, on the extremity of a brass frame annexed to the interior stand; which frame is capable of being raised or lowered at pleasure by a milled-headed screw; so that the wire can, at any time, be released from the weight of the plummet, by screwing up the vessel containing it.

There are two arches attached to the end of the tube, one on each side of it, and firmly united together by means of brass pillars; which arrangement effectually secures the divided arch from injury. The total extent of the arch is about 15°, having half of its subtense on each side zero. It is divided into every 5 minutes; the micrometer-screw measuring any supplementary quantity. Golden pins were let into the arch, by the advice of the Astronomer Royal, on which the divisions were laid off by Mr. Berge, in a very masterly and accurate manner, as will be seen hereafter. A magnifier, whose focal distance is about half an inch, is placed under the bottom of the cross piece opposite to the arch, and is furnished with a horizontal adjustment for bringing it directly over the plumb-line.

Among the various eye-pieces, of different magnifying powers, is one furnished with a prism. This, necessarily bent at right angles, enables the observer to see the stars without touching the frame. The use of it has been found convenient; but habit and proper caution enable the astronomer to use either of the other glasses. Having given this cursory and general description of the instrument, as seen at first view, I shall proceed to an explanation of the plates, which show its various parts.

Particular Description of the Zenith Sector.

Plate X. Represents a general section of the instrument and stand. AB is one of the four great uprights of the external mahogany frame, and CB its top, having an opening in D, for admission of light. The uprights consist of two strong pieces, firmly screwed together; each upright having seven strong screws, as seen in the upright AB. The top may be considered as a sort of square table, screwed down on the upper part of the frame. Between each of the two uprights is a brace, diagonally fixed, for strengthening the stand, as may be seen in the plate; and four others go horizontally, from upright to upright, for the purpose of still farther strengthening the whole. Across the bottom of the frame, and exactly in the middle of it, is a very strong mahogany plank, whereon rests the sector, having a stout straight edge bar of the same substance underneath. In the middle of this cross piece, as seen at E, is an apparatus of brass, furnished with an azimuth circle, having a hollow receptacle of bell-metal in the centre, in which rests, on a conical point, the interior mahogany frame FGHI. This brass work, which is strong and substantial, may be seen in Plate XIV. It is there represented *in plano*, with the bottom part of the interior stand

placed above it. The means of making the interior stand vertical, are found in the work annexed to the azimuth circle. They consist of two screws, attached to two plates of brass, placed at right angles to, and also flat on each other. S *s* (Plate XIV.) are the screws. A vernier on the divided circle may be seen at S; and at *s*, the method of clamping the bottom of the stand. On the opposite side is another provision for clamping this stand, when the face of the sector is changed from east to west, or *vice versâ*.

KLMNO*p* (Plate X.) is a section of the telescope and axis, MR, MR, being two of the four braces for strengthening the axis, and steadying the telescope. K is the place of the eye-tube; L the elliptical reflector for illuminating the wires at K; and ON a hollow cylinder of brass, independent of the tube. In the upper part of this cylinder, the object-glass is rivetted; the cylinder itself being fastened to the great eye-tube, in a permanent manner.

W, W, are two weights, hanging freely from the ends of two levers, the opposite ends being furnished with four friction-wheels. The points of support, between the weights and wheels, are at TT, being at the extremities of two upright solid pieces of metal, which are moved up or down by the screws beneath them. These counterpoising weights prevent any bending of the axis, between the pivots and those parts to which they apply. The apparatus for carrying the levers, is attached to the inside mahogany frame by screws, as represented in the section. See also Plate XIII.

The plummet and plumb-line are seen at *aed*; the point of suspension being *a*, and the plummet *d*; the plumb-line passing close by the arch, whose section is *bc*, and also near to the dot

or small circle *e*, described on the thin slice of mother-of-pearl, shown in the section of the axis at *e*.

A lamp is attached, or rather rests, on circular supports annexed to the side of the interior frame, and may be seen at XZ. At the back of the lamp, placed in a recess, is a concave reflector at Z; and, in the front of it, a tube running out to X, having a double convex glass at P, for throwing the light on L, which first passes through a double concave glass in the side of the telescope, and then, from the reflector L, is thrown down on the wires near K. The concave speculum Z, has two adjustments for converging the reflected light on the little elliptical speculum *b*. This last-mentioned speculum throws off the said light at *b*, which passes into the axis at G, illuminating the mother-of-pearl at *e*, and, finally, is transmitted out of the axis at *p*.

klmn, is a section of the long microscope, for conveying the image of the dot and wire, sent out of the axis at *p*, to the eye at *k*. This microscope is firmly attached to the side of the frame, by brass cylinders, *kk*, *ll*, *mm*, *n*, and has one plano-convex glass at *q*, a prismatic eye-glass at *u*, and a metallic reflector at the top, *o*. At the upper end of this long microscope, and directly behind the speculum, is a screw, by which the reflecting metal is brought into one of its requisite positions. The other adjustment of that metal is performed by two screws, which apply to the sides, and give it lateral motion. The plano-convex glass at *q*, is rivetted into the head of a long tube *uq*, which slides up the microscope. The upper part of the microscope at *o*, is placed exactly opposite the end of the axis, in a very firm way.

The rod for giving motion to the plumb-line, is *vwx*; *v* being the top of it, *w* the place of the universal joint, which separates the two parts of the rod, and *x* the bottom of the rod

itself, to which part the hand is applied. In this section, th top v is not furnished with the pipe connecting it with the ber lever above; but the representation of it, together with the leve itself, and accompanying spring, will be understood by referrin to the plate which contains a representation of those parts.

In Plate XII. is a section of the axis passing through th pivots, and one exhibiting the face of the several united planc constituting the diaphragm, which adjust, in every directio the slice of mother-of-pearl. Above this latter, is a view of th lever which gives motion to the plumb-line, with the pipe spring, &c.; these are represented as seen from an eye at on of the pivots; and, above the other section, is a view of the sam apparatus, seen in a direction at right angles to the former one.

In the latter of the above-mentioned sections, *bab* is th diaphragm, having in the middle, as at *p*, a circular piece o mother-of-pearl, extremely thin, with a small dot in the centre This brass work is annexed to the large end of a hollow conica piece of brass, which exactly fits the axis at its proper place It is there screwed fast, and may be considered as of one piec with the axis. In the adjoining figure, *bfkl*, is a section of th cone, *p* being the place in the opening of the brass work whicl receives the mother-of-pearl. In the representation of the dia-phragm, *a* and *b* are two screws, at right angles to each other and respectively attached to flat pieces of metal which slid on each other. If *fb* represent the plumb-line, the direction o each screw is that of an angle of 45 degrees with it. By mean of these adjusting screws, the dot or little circle at *p* was place in its proper position, or in the centre of the circle *abb*; so that on an adjustment of the plumb-line, in any one position of the instrument, the dot still remains accurately bisected, howeve

the telescope be subsequently moved. Above *abb*, is a small frame work of brass, from which the plumb-line depends: it is attached to two of the four horizontal tubes on which the Y frames are fastened. *cd* is the pipe fastened to the end of the rod; the end of this pipe has a screw, which passes through a nut, and acts against the end of the lever *d*, whose centre of motion is *g*, and whose other extremity is *f*, where there is a small piece of hard steel with a notch, for the reception of the plumb-line *fb*, suspended from *g*. Against the upper surface of the arm *gd*, a helical spring continually presses downwards; it is fastened above the end of the lever, at *e*; by which means, the arm *gd* is constantly pressed against the end of the pipe, obviating the possibility of any play or shake of the lever, round its centre *g*.

The same figure contains an elevation of the frame-work just described, as seen by an eye in the plane of the diaphragm produced. It is necessary that it should be closely inspected, for the purpose of obtaining an adequate idea of its construction. In this figure, *xv* is a small cylinder, with a screw and loose collar at the end *v*, for fastening the plumb-line, which goes over the notch *n*, and passes through a hole in the upper part of the axis at *f*, and out again at *b*, almost touching the mother-of-pearl at *p*. *sru* is a strong spring, fixed at *s*, through the middle of which, at *r*, passes a screw, which is, in fact, an adjusting screw, for bringing the plumb-line close to the dot on the surface of the diaphragm; and here it is necessary to observe, that the plane of the divided arch and that of this diaphragm, are one and the same when produced. There is no part of the instrument more complete than the apparatus for suspending the plumb-line, and that which regards the dot. I

shall now return to a farther consideration of the construction of the axis.

In Plate XII. there is also a horizontal view of the upper part of the axis; A being the head of the microscope, and B the little diagonal speculum, for throwing the light on the diaphragm. C is the opening in the axis above the object-glass; and D a brass slider, for covering the opening at C. E and F are two pulleys, attached to the side of the axis, over which pass two strings, having their ends united in opposite points of the shutter D; the other two ends of the string being within reach of the observer, who by this means easily opens or shuts the slider.

In Plate XIV. ABCD is the moveable frame, fastened to the top of the external stand, and having an octagonal opening at E, for receiving the brass work connected with the four horizontal pipes carrying the Ys. The touching points between the octagon and cylinder, are *g*, *h*, *n*, at which parts of the frame hard pieces of metal are inserted. To prevent all possibility of shake in the cylinder, which would render an adjustment of the instrument troublesome, if not impossible, there are two strong screws at *m* and *n*. One is a helix, which acts against *m*, and against the end of the sliding piece *n*; so that, by a condensation of the helix by the screw *m*, the piece *n* acts against the head of the cylinder inserted in E.

Plate XI, represents an elevation of the instrument seen sideways, and is that part to which the long microscope is attached: it serves to show the formation of the interior stand carrying the sectorial tube. A B C D E F are mahogany uprights, firmly united at the bottom and sides by proper cross pieces, and at top by the plate of metal *a b c d*, through the ends of which pass those of four horizontal pipes, the plate *a b c d* being one which

carries a Y, hidden in this elevation, by the upper part of the head of the microscope.

In the middle of the cross pieces, which unite the side of the frame to its corresponding one, are two wheels with long axles, as before mentioned. In this elevation they are seen at B and E; and have strings passing from them to their respective sides of the tube, where they attach to the pins P, P. At the ends of the axles nearest to this elevated side are two other pulleys. In the view which this plate affords, these wheels are projected against the others just spoken of; but their uses will be more readily understood, on perceiving the strings which pass over the upper pulleys, and afterwards sustain the weights W, W, in their upright cases.

In this elevation is seen the telescope attached to the side of the great tube: it is used when the instrument is got into the plane of the meridian. The vessel for receiving the plummet is seen at V; and at S the adjusting-screw, for elevating or depressing the frame which supports it. LL is the clamp-arch, supposed to be attached to the other side of the tube, or that which supports the lamp. At the bottom of the stand is seen the azimuth circle, and the apparatus belonging to it.

For the purpose of conveying a clear idea of the arrangement of the lower pulleys, and the manner in which the two arches are joined to each other at the end of the telescope, there is given, in Plate XII. a horizontal view of the same. The vessel for receiving the plummet, its supporting frame, and the magnifier for viewing the dots or points on the divided limb, are likewise represented. The body of the telescope is here taken away, leaving nothing more than the plate at its end, with the contiguous work belonging to the wires.

To show with distinctness and sufficient perspicuity, the manner in which the cross wires are sustained in the tube, also the means by which they are adjusted, figures are given in Plate XII. These also show the micrometer-screw, and the mode of clamping it to the proper arch. In the horizontal representation of the end of the telescope, the wires are seen in the centre, and also the two screws, with the helical spring for adjusting and retaining them. EF is a strong brass arch, with an edge bar IK, placed parallel to the divided arch. At EF are seen two milled-headed screws, passing through a metallic embracement of the bar and arch, which are firmly connected with the apparatus belonging to the micrometer by their means. At S is a piece of hard polished steel, against which the point of the micrometer-screw rests; and, as the arch EF is the segment of a circle whose radius is its distance from the axis of the sector, the point of that screw always buts against the steel head in the same place. In this plate is also seen a vertical section of the same parts. Here, EF is the back arch, and MS the micrometer-screw. This figure also shows the means by which the pieces carrying the wires, and inserted into the end of the telescope, are retained in their proper places. CA and DB are two long pillars, which pass through an annular piece of brass parallel to the end of the tube.

A screw with a windlas-like head is seen at G, from the turning of which, the wires are moved in one of their proper directions. A screw for giving them a motion at right angles to that obtained by the fore-mentioned one at G, is seen in the horizontal view of the end of the telescope at H.

It would be swelling this account to an inconvenient size, if I were to attempt any farther explanation of the plates; I shall,

therefore, close this article with a few observations on the manner of adjusting the instrument for observation.

Manner of adjusting the Instrument for Observation.

The feet of the external stand should be first carefully brought into a horizontal plane; and, when they are so, the azimuth circle will be, necessarily, parallel to it, having its centre under the middle of the opening in the mahogany frame screwed on the top of the stand. This being done, and the instrument set up, the plane of the arch should be brought parallel to one of the sides of the stand, in which situation, the internal frame is to be clamped to the azimuth circle, and the wire brought to its proper distance from the limb, by means of the adjusting-screw attached to one of the sliders, which carries the concave receptacle and conical point. The dot at zero should then be brought exactly under the plumb-line, as seen through the magnifier, and the point on the micrometer-head, at which its index stands, noted. The instrument is then to be turned half round; and, if the same dot on the arch still continues bisected, it will afford a proof of the internal stand being upright in one direction. But, if the dot should not continue bisected by the plumb-line, it must be made to do so, and the revolutions, or parts of a revolution, counted; half of which is to be turned back on the micrometer-head. The same dot, zero, is then to be brought under the wire, (plumb-line,) by means of the other adjusting-screw beneath the azimuth circle. If the stand is pretty accurately set up, one operation is sufficient for bringing the interior frame upright in one direction, viz. either in that of the meridian, or the one at right angles to it. The arch is then to be turned

round 90°, and the same operation gone through. This being properly done, the interior frame is made perfectly upright.

The next step to be taken, is that of placing the long level on its axis above, and rectifying that axis by means of the Y plate screws. If this be done carefully, the bubble will remain between the pointers of the level, whatever position the sector may be placed in. Having thus rectified the instrument, by making the internal frame upright, and the axis horizontal, the only remaining point to engage attention is, placing the plumb-line at a proper distance from the arch: this is done by means of the screw acting on the spring just under its point of suspension. If great care be used in going through these several adjustments, the instrument may, at any future time, be accurately adjusted for observation, by turning the proper screw belonging to the azimuth circle, and bringing the arch to its usual distance from the wire.

Laying off the Points, or dividing the Limb of the Sector.

The first step preparatory to finding the length of the radius, was to mount the sector in its frame, and adjust the counterpoising weights. By attention to the proper points on the levers, the axis was kept from bending, the pivots not having more than a weight of two pounds to support. This done, a tool with a well defined point was made to press lightly against the face of the arch, and firmly sustained in that situation, while the sectorial tube was slowly moved to the right and left: a fine line was by this means described on the limb, passing through the centres of the golden pins. The arch having been thus

edge of a very strong plank, having its axis horizontal, and the pivots resting in Ys firmly fastened in the middle. The end of the arch, whose face became vertical, was supported by a brass plate screwed down on the end of the plank. When the pivots were placed in the Ys, and the telescope sustained in several places, to keep it from bending, a brass slider, on the surface of the plank, was moved till the line before mentioned coincided with that described on the arch. The telescope was then quickly taken out of the Ys, and, as speedily as possible, brought into a similar position on the other side; for which purpose, braces to support the tube had been previously prepared. By these means, twice the length of the radius was obtained; proper care having been taken to have the Ys so placed, that the centre of the pivots should be in the same plane with the two sliders at the extremities of the plank. The distance between the lines was then measured, and $\frac{1}{16}$ taken for the chord of 7° 10′, which was immediately laid off on the face of the sector, on both sides zero.

Although little doubt could be entertained of the truth of the total arc thus assumed, yet, that the length of its chord might be compared with that derived from the usual modes of operation, Mr. BERGE (as proposed by Mr. RAMSDEN) prepared a brass arch, which he let into a frame, on which, after striking a portion of a circle with the radius obtained as above, he laid off the chord of 60°. This arch he divided by continual bisection, till he obtained the chord of 7° 10′, which he compared with the same angle laid off on the sector's limb. He had the satisfaction of finding no perceivable difference; and, that there really existed none, was denoted by the unresisted fall of the points into their respective holes. The arch of the sector was then divided

into degrees, and every degree into five minutes; and the holes were afterwards opened with a tool made for the purpose. As gold pins had been let into the arch, Mr. Berge was enabled to go through the division of it with great success, and afterwards to enlarge the holes, without destroying his accurate work. The observations will offer a more satisfactory testimony of the credit due to his abilities as a workman, than any opinion which I might express myself as entertaining, although founded on the same *data*. It remains for me only to observe, that I think he has delivered this instrument into my hands without any imperfection of execution; and that I believe it would not have been superior, had the ingenious inventor lived to complete it.

Adjustment of the meridional and horizontal Wires.

After the arch was divided, the axis of the telescope was laid on a pair of Ys connected to a firm support, and made nearly horizontal. The tube was then brought up to a level with the axis, and sustained at proper intervals, whilst the end of the telescope rested on a small piece of metal connected to a fixed bar, by means of an adjusting-screw. This end was then moved, till an object sufficiently small, (a speck or dot,) at a proper distance, appeared nearly in the centre of the field. The telescope was then properly secured from bending, and rendered perfectly steady, but admitting of a small motion sideways, the Ys having also a corresponding adjustment.

A microscope, furnished with a moveable wire, was then fastened to a beam attached to the brick wall, and its end brought close to the edge of the arch of the telescope. Upon this edge, as well as on that of the other arch, Mr. Berge had address to lay off a point, very nearly in that place where the

plane passing through the axis and zero cuts the arches. This being done, the telescope and Ys were moved laterally, till the vertical wire bisected the speck. The system of wires was then turned, till the meridional one was made exactly perpendicular to the axis, as seen from the mark being bisected in every part of the wire, when the end of the telescope was moved up and down by the adjusting-screw. The axis was then carefully taken out of the Ys, and inverted: it was afterwards placed as before, and the distance between the spot and vertical wire estimated by the eye. The telescope was then moved in azimuth, half that quantity, and the meridional wire brought to a bisection on the speck. Repeating this operation twice or thrice, the vertical wire became accurately perpendicular to the line passing through the centre of the conical axis, and also in the plane passing through the centre of the tube.

The next step was, to move the whole system of wires in the direction of the perpendicular, in order that the horizontal one (at right angles to the vertical wire by construction) should be also brought into its proper position. For this purpose, the telescope was moved a little in azimuth, and the proper wire made to bisect it accurately, at which time, the wire of the micrometer before mentioned was brought over the dot on the edge of the limb.

In this position of things, the instrument was taken off the Ys, and turned over; it was then again carefully placed in its former position, and the end of the telescope brought up by the adjusting-screw, till the distant speck was bisected by the horizontal wire. Now, if this horizontal wire had been, by accident, placed so that the point of intersection of the two wires was

exactly in the true centre of the telescope, the dot on the edge of the other limb would have been bisected by the wire of the anterior microscope. This was found not to be the case; but it was made to be so, by halving the differences, and moving the horizontal wire so as to bisect the mark. After this had been again examined, the vertical wire was examined, when it was found necessary to go through a part of the operation a second time. This was to be expected; but the wires were, by these means, at last properly placed, and guards were then fixed over their adjusting-screws. I shall now proceed to speak of the use I made of this sector in the year 1802.

Particulars relating to the Operations of the Year 1802.

I have already stated, that a proper observatory had been provided for the reception of the zenith sector. The dimensions of it were twelve feet square at bottom, and six feet square at top; its proportions being the same as those of the external stand. A floor having a square vacuity, to admit of the instrument standing on the ground, covered the joists of it. The sides of the observatory were of strong painted canvas; and the roof of wood, with an aperture, which could be opened or closed at pleasure, for viewing the stars near the zenith.

The instrument, with this observatory, was erected in the Tower on the [illegible] of April, merely to examine all its parts, and [illegible] any thing could be done to render it more [illegible]. Some [illegible] *addenda* were accordingly made, and the whole [illegible] perfect, was removed to the Royal Observatory, and erected in the garden of [illegible] Astronomer [illegible] close [illegible] of the [illegible].

I am now to specify, that my intentions were to devote, from this period, the whole or the greatest part of the following summer, to the use of this sector; nor did I indeed imagine such a portion of time more than sufficient. I purposed to erect it at Dunnose, and at Clifton, the extremities of my arc; and also at Arbury Hill, near Daventry, the station almost in the middle of it. This last station I fixed upon, because it was proper to ascertain how far the observations for determining the extent of the whole arc, would agree with any others made for finding the value of its parts. The erecting of it at Greenwich was necessary, for the purpose of observing the zenith distances of certain stars, which were afterwards to be observed at Dunnose, thereby affording means of ascertaining the latitude of that station.

The instrument remained at the Royal Observatory till the 26th of April; and, although the weather was for most part of the time unfavourable, yet the erecting of it there will be found, as appears in a future part of this work, to have answered the proposed end. One very material service accrued to myself; this was, the advice and instruction I received from the Astronomer Royal, for the successful management of the sector, by which I scrupulously governed myself throughout the whole of the subsequent campaign. Having observed the zenith distances of some few stars, and made myself completely master of every adjustment about the instrument, the sector, with all its apparatus, was sent to the Isle of Wight, by way of Southampton; every possible care being used to protect it from injury, not only while transporting by land, but also when under the act of being taken in, and removed out of, the vessel which conveyed

it from that place to Cowes. It will be readily supposed, that watchfulness and care were necessary, to preserve this complicated instrument from being damaged by accident or roughness of the roads.

In the year 1794, an iron cannon was sunk in the ground, for the purpose of permanently preserving the point on Dunnose, where the direction of the meridian was observed in 1793. It must be now remarked, that the cannon so inserted could not have its breech placed so low as might have been wished; in consequence of which, it became necessary to erect the observatory for the reception of the sector some little distance southward of the old station. The distance from the centre of the gun to the point over which the instrument was afterwards erected, was six feet and a half.

To procure for the external stand, and thence for the whole apparatus, a firm foundation, I caused four long stakes to be driven into the ground, one for each foot of the stand, to which its feet were firmly screwed down. The surfaces of the stakes were then cut off smooth, and brought into the same horizontal plane, by which means, the interior frame and sector were placed much within the limits of their several adjustments.

The pointed top of Sir Richard Worsley's obelisk afforded me an excellent means for bringing, with the assistance of the side telescope and azimuth circle, the plane of the arch into the true meridian. The distance and magnitude of that object is extremely convenient for the purpose. Its bearing from the meridian of the station is 87° 42′ 33″, as I shall show in its proper place. The side telescope was turned to this object very frequently: and I never found the vernier, on the azimuth circle

to indicate any serious warp in the stand. Its greatest variation was 4′; but, for several days together, it did not amount to 30″.

The weight of the plummet, I adjusted to the strength of the plumb-line, in the usual way. I suspended it in air, and gradually increased its weight, till the wire broke. This plummet was then immersed in the vessel appropriated for its reception. It will, perhaps, not be improper to observe, that I was careful to give the plummet its maximum of weight, that its wire might not be subject to motion from streams of air.

As it was to be apprehended that errors would result, from the effects of an inequality of temperature in the air within the observatory, I placed two thermometers, both adjusted to a third, near the telescope. One I elevated as high as the axis, the other I laid on the hollow brass cylinders which connect the divided arch with that behind it, usually called the back arch. In the day, I found (as may be seen in the register of observations) the heat a little greater at the top of the tent than towards the bottom; and the reverse was generally the case at night.

To equalize the temperature at those times when the sun shone out, or the weather was hot, I opened the shutters in the roof, as well as the door of the observatory, a considerable time before the moment of observation. By these means, the air within the tent was rendered tolerably uniform in its degrees of heat. For the space of a week following the commencement of my observations, I suspended a third thermometer from the milled-headed key which turns the diaphragm placed inside the telescope. As the situation of this thermometer was midway between the two others just mentioned, I always found the temperature there, a mean between those degrees shown by the

upper and under thermometers; and as, in the course of the time specified, I had various opportunities of satisfying myself on this point, I desisted from making any farther use of it. For the purpose of ascertaining the limits of the errors likely to result from the cause now spoken of, it will be right to institute some little inquiry into its mode of operation.

In Plate XVI. Fig. 1, let CD be the line passing through the centre of the sectorial tube, brought into any position for observation; the angle made with the zenith being ACB, and CA the consequent direction of the plumb-line. CB and BA may therefore represent the radius and arch of the sector, when in a state of uniform temperature throughout.

Now, at any time, let the thermometer at the top C, indicate a degree of heat superior to that shewn by the other at B; and let it also be supposed, that the difference between those degrees of heat, at any intermediate point, is directly as the distance of that point from C or B; and farther, let the tube CB be extended to D, while the arch AB continues of the same length.

If the line CA be extended to F, and the line AE be drawn parallel to BD, meeting the arch FD in E, then will the small space FE measure the error in the observed zenith distance of the star.

As the angle ACB must in all cases be small, ACB and EF may be considered as two similar sectors of circles; under which supposition, we get $FE = \frac{AB \cdot EA}{CB}$; and, applying this to an extreme case occurring at Arbury Hill, on the 12th of September, we get $AE = \frac{5° \times 0{,}0001237 \text{ inches } \times CB}{2 \times 12}$, hence $FE = \frac{5° \cdot \times 0{,}0001237 \times 6° \cdot 26'}{2 \times 12}$ $= 0'',596$.

As few of the stars selected for observation were, at either of

the stations, so far from the zenith as 6°, it is obvious little inaccuracy can have resulted from the difference of temperature here spoken of; and this supposition will receive farther support, from the actually near approach of the two temperatures to an equality with each other, as appears by taking the mean results of the last two columns in the register of observations. That the scrupulous mind may be satisfied in this particular, I shall insert, in its proper place, a table for supplying the correction arising from this cause; as the effect of a greater heat in the upper part of the tent is an error in excess, so a reverse of the case produces one in defect.

On the first convenient opportunity, I measured, with great care, the distances between every successive set of dots on the divided arch, contained between zero and 7° 10′. This was done at a time when the thermometers denoted a perfect uniformity in the temperature of the air within the tent, and when, from the calmness of the day, no streams of air could affect the plumb-line. Although I had, previously to the performance of this matter, perfectly satisfied myself that the rays of heat, emitted from the lamp illuminating the face of the arch, do not expand it perceivably, yet I thought it best to wait for a day when the strength of the light should enable me to discover, and properly bisect, the points, without the aid of that lamp. Between zero and 7° 10′, on the left hand arch, I found there were 430 revolutions of the micrometer-screw + 38,2 divisions; and, between the same point and 7° 10′ on the right hand, 430 revolutions + 39,2 divisions.

From this it appears, that the mean value of one revolution of the screw, is 0′ 59″,098. Mr. Berge endeavoured to place the arch, carrying the apparatus of the micrometer, so that one revolution

of the screw should be exactly a minute. On trial, he found it nearly a second short; for which reason, he divided the head into 59 parts, and called each of them a second. I think it proper to repeat the observation, that the two arches were measured with the greatest care, because it admits of the remark, that every space subtending 5′ was measured with the *same part* of the screw, beginning very nearly from 9 on the index. This instrument will, at a future period, probably pass into other hands; it may therefore be right to state, that I found, from an examination of the screw, an error of nearly 1″, in the part contained between 17 and 19 on the index, arising from a small notch which, with a magnifier, I could plainly perceive on one of the threads. As it cannot but be the general wish to have some evidence of the accuracy with which this sector has been divided, and also how far I have succeeded in the performance of what is now under consideration, a table will be given, in which the value of every 5′, in the first degree on each side zero, will be found in revolutions and parts of the screw.

Having towards the end of June found my observations sufficiently numerous, and apparently sufficiently accurate, from the regular differences subsisting amongst them, I took down the sector, and, with every thing belonging to it, repaired to Clifton, the northern extremity of my arch. The instrument arrived there in safety, on the 20th of July; and, as the direction of the meridian had been previously determined, the instrument was immediately set up, and made ready for use.

At this station, Laughton spire afforded me an excellent mark for adjusting the instrument in the plane of the meridian. The bearing of it is 1° 56′ 12″ south-west; and, from my being able to see it in the observatory, without rolling up either of its

canvas sides, I had ready means, at all times, of turning the telescope to that object. And I can take upon me to say, that during the whole of my stay at this station, I never found the instrument out of the plane of the meridian more than half a minute.

Of the 27 stars observed at Dunnose, 17 were observed at this station; they were the following, *viz.* β, γ, 45*d*, 46*c*, 51, 16, μ, Draconis; 1κ, 10ν, Cygni; η, ξ, Ursæ; 22τ, 85ν, 52, υ, Herculis; α Persei, and Capella.

As the weather for most part of the time proved favourable, the observations were completed on the 22d of July; and, as there appeared to be sufficient time, between that period and the arrival of the season which would necessarily terminate my operations, to carry on my meridional line to the Tees mouth, I reconnoitered the country in that quarter, and selected the stations all the way between it and Clifton.

On the 23d of July, the instrument and observatory were taken down, and the large theodolite erected over the point. White lights were sent to the distant stations, and were all observed, except those fired on the 30th day of the same month; and, as the night on which those lights were burnt was remarkably clear, and it was therefore probable that some intervening land obscured the distant hill, I desisted from making any farther attempt towards the execution of the above scheme, as any greater loss of time might prevent me from making the proposed observations on Arbury Hill. I therefore sent the sector to this last-mentioned place, where it arrived on the 3d of September, and was erected on the 7th, the direction of the meridian having been previously ascertained, by two double azimuths of the pole star. But it is proper I should observe,

that the sector was not set up over the old station, as injury to some amount would have been sustained by the person farming the soil, owing to its cultivated state. The spot on which I fixed, was 34 feet to the north, and 28 towards the west, of the former station.

Of the stars seen at Clifton, 12 were observed at Arbury Hill. These observations were continued, with very little interruption, till the 4th of October, when the party, with all the apparatus, returned to London; the zenith sector being found as perfect on its return as when first sent into the field, a circumstance inferring both the strength and perfect union of its parts.

Particulars relating to the Measurement of a new Base Line, on Misterton Carr, in the Year 1801.

The apparatus used for the measurement of this base, was the same as that employed on Hounslow Heath, Salisbury Plain, and Sedgemoor; and the like pains were taken to ensure its accuracy, as were used on those occasions. The points for lining out the base were put into the ground with great truth and precision; the large theodolite being used as one of the means, and in the same way as in measuring the base on Salisbury plain. Previous to the commencement of this operation, two large blocks of oak, with square holes on their upper surfaces, were sunk in the ground, at the extremities of the base; the point of intersection of the diagonals of each hole, severally denoting them. These diagonals were drawn on lead, cast into the holes, and ground to a smooth plane, even with the surface of the block.

Before the measurement began, the working chain A, and the

50-feet chain, were both compared with the standard B. For this purpose, a calm cloudy day was waited for, which opportunity presented itself on the 2d of June. The pickets for the registered heads were then driven into the ground a considerable depth, and the coffers laid in a right line between them. The chain A was then laid out perfectly straight; and five thermometers, equally distant from each other, were put close to its side, their temperatures being as follows.

Thermometers.

1 —	2 —	3 —	4 —	5.
67°	65°	67°,5 —	67°,5 —	67°,5.

The chain A was then taken out of the coffers, and B laid out in its stead. The difference of their lengths, which was measured with the micrometer-screw, was found to be 1 revolution 6 divisions, *viz.* A longer than B; the temperature remaining constant the whole time of trial. In the course of the day, the same operation was repeated, the five thermometers standing at 69°,5—69°—69°,5—69°—69°, when B was found to be 1 revolution $6\frac{1}{2}$ divisions of the micrometer-head shorter than A. Therefore, the mean, *viz.* 1 revolution $6\frac{1}{4}$ divisions, was considered as the true difference of their lengths. The length of twice the 50-feet chain was, at this trial, found to exceed that of B, 2 revolutions 4,5 divisions; which is nearly the same determination as formerly resulted, from a comparison of the chains with each other on Sedgemoor. It may be seen too, by referring to the account of the measurement of the base on that spot, that the difference between the lengths of the standard B and common chain A, was nearly the same at that period as now; the difference being 1 revolution 7 divisions. I therefore concluded I might,

with safety, suppose the length of the standard chain B to be exactly the same then, as at the period when Mr. RAMSDEN compared it with the points inserted into the cast iron bar, mentioned in the first account of the trigonometrical operations.

The measurement of this, the fourth base, commenced on the 6th of June; and was continued, without much interruption from bad weather or other causes, till the 28th of July, when it concluded with the 263d chain, the overplus, 38,321 feet, being carefully determined, by means of a silver wire and pointed plummet let fall over the point marking the north-west extremity of the base. The two chains were then carefully compared with each other; when it was found, that the wear of the chain A was exactly one division on the micrometer-head, or $\frac{1}{260}$ part of an inch. As the length of this base is nearly the same as that on Sedgemoor, it was reasonable to suppose that the elongation of the chain, by the working of the joints in each measurement, would be found the same, provided no injury had taken place from accidental circumstances, or rusting of the pivots and holes, during the time the chains were laid up in the Tower. After the reduction of the base, I shall have occasion to show that my ideas were correct in this point, as Mr. BERGE has lately remeasured both chains.

Angles of the great Triangles observed in the Years 1800, 1801.

At Beacon Hill.

Between	°	′	″	Mean. ″
The north and south end of base -	20	47	19	19,75
			20	
			20,25	

Between	°	′	″	Mean.
North end of Base and Gringley -	34	44	40,75	″
			42,25	42,25
			42,75	
			43,25	
Gringley and south end of Base -	13	57	22,75	
			24	24
			24,25	
			25	
Heathersedge and Gringley -	138	9	15,5	
			16	16
			17,5	
At North End of Base.				
Beacon Hill and south end of Base -	60	17	16	
			16,25	16,5
			17,25	
Beacon Hill and Gringley -	74	46	55,5	
			56,25	56,5
			57,25	
			58	
At South End of Base.				
Beacon Hill and north end of Base -	98	55	26	
			27	27,5
			28	
			29	
Beacon Hill and Gringley -	114	51	31,5	
			31,75	
			32,5	32,5
			32,75	
			33,75	
At Gringley.				
Beacon Hill and south end of base -	51	11	5,25	
			5,75	
			6,75	6,5
			7,25	
			7,5	

Between	°	′	″	Mean ″
Beacon Hill and north end of Base -	70	28	21,5	
			21,75	22,2[illegible]
			22,5	
			23,25	
Beacon Hill and Heathersedge -	23	10	5	
			6	6
			7	
Sutton Ashfield and Heathersedge -	46	20	23,5	24
			24,5	

At Heathersedge.

Beacon Hill and Gringley - -	80	40	37,75	
			38,5	38,5
			39,25	
Sutton Ashfield and Gringley -	54	52	36,5	
			37,5	37,5
			38,5	
Orpit and Sutton Ashfield - -	39	8	37,25	
			37,75	
			38,25	38,7[illegible]
			39,5	
			39,75	

At Sutton Ashfield.

Heathersedge and Gringley - -	78	47	1,25	
			2,25	2
			2,5	
Orpit and Heathersedge - -	60	22	24,5	
			25,5	25,5
			26,5	
Hollan Hill and Orpit - -	118	49	8	
			9	9
			10	

At Orpit.

Between	°	'	"	Mean. "
Heathersedge and Sutton Ashfield -	80	28	56,25	57,25
			56,75	
			57,75	
			58,25	
Hollan Hill and Sutton Ashfield -	21	27	19,5	20,5
			20	
			20,5	
			21	
			21,75	
Bardon Hill and Hollan Hill - -	62	8	24,5	25
			25	
			25,5	
Castle Ring and Bardon Hill -	56	3	13,75	14,75
			14,75	
			15,75	

At Hollan Hill.

Between	°	'	"	Mean. "
Sutton Ashfield and Orpit - -	44	43	30,75	32
			32,5	
			32,75	
Bardon Hill and Orpit - -	74	52	36,25	38
			37,75	
			38,75	
			39,25	

At Bardon Hill.

Between	°	'	"	Mean. "
Hollan Hill and Orpit - - -	42	58	58,75	59,5
			59,25	
			59,75	
			60,25	
Castle Ring and Orpit - -	68	24	3,75	4,75
			4,75	
			5,75	
Corley and Arbury Hill - -	38	25	12,5	13,25
			13,25	
			14,25	

At Castle Ring.

Between	°	′	″	Mean.
Bardon Hill and Orpit - - -	55	32	43	
			43,25	
			43,75	″ 44
			44,25	
			44,75	
			45	
Corley and Bardon Hill - -	47	54	40,5	
			41,75	
			42,5	42,25
			43,25	
			43,5	
At Corley.				
Castle Ring and Bardon Hill - -	72	32	45,75	
			46,25	46,5
			46,75	
			47	
Arbury Hill and Bardon Hill -	107	20	13,5	
			14,25	14,25
			15,75	
At Arbury Hill.				
Corley and Bardon Hill - -	84	14	32,5	
			33,25	33,5*
			34,25	

Reduction of the Base to the Temperature of 62°.

	Feet.
The apparent length of the base was 259 chains of 100 feet each, + 8 chains of 50 feet each, and the overplus of the last chain *viz.* 38,321 feet -	26338,321

The chain B, before the measurement, was found to be 16¼ divisions on the micrometer-head shorter

* For the observations of the angles of the triangles southward of Arbury Hill, see the Philosophical Transactions for 1795 and 1800.

	Feet.
than A, the length of which, according to Mr. RAMSDEN's determination, may be taken $=$ 100 feet $+$ 0,1236 inches, in the temperature of 54°; which gives A 0,12363 parts of an inch too long. Therefore, if to this is added half the wear, *viz.* 0,00192 parts of an inch, we shall get $\frac{0,12555}{12}$, which $\times$ 259 gives 2,709 feet, which add - - - -	$+$ 2,709
The 50-feet chain, before the measurement, was compared also with B, and found to be 24 divisions on the micrometer-head longer; therefore, $\frac{0,0943}{12} \times 4$ $=$ 0,0314 parts of a foot, which likewise add -	$+$ 0,031
Again, the sum of all the degrees shown on the thermometers was 98083, wherefore, $\overline{\frac{98083}{5} - 54^\circ} \times 263,38 \times \frac{0,0075}{12} = 3,3713$ feet, is the correction for the mean heat in which the base was measured above 54°, the temperature in which the chains were laid off, and this also add - - - -	$+$ 3,371
Finally, for the reduction to the temperature of 62°, or 8° on the brass scale, we have $\frac{0,1237 \times 263,38 \times 8^\circ}{12}$ $=$ 1,720 feet, which subtract - - -	$-$ 1,720
Hence we have the true length of the base, in the temperature of 62°, $=$ - - -	26342,712

The surface of the ground on which this base was horizontally measured, is said to be not more than 35 feet above the surface of the sea, in the mouth of the Humber, at *half tide.* And, although it may not perhaps be a very correct deduction, yet, as I understand that conclusion arose out of a levelling operation, it may be taken for granted that we shall not err, as

to sense, in our conclusions, if we consider Misterton Carr as situated on the mean surface of the spheroid. I shall, therefore, take 26342,7 feet for the true length of the base; and I think it cannot exceed or fall short of that quantity, more than two inches.

Recent Comparisons of the standard and working Chains, with the points inserted in the cast Iron Bar.

In the reduction of the foregoing base, I have taken it for granted, that the standard chain is precisely of the same length as when it first came out of the hands of Mr. RAMSDEN. Circumstances which need not be mentioned in this part of my paper, but which, in their proper places, will be explained, have induced me to get both the long chains remeasured. Mr. BERGE, therefore, at my request, prepared the bar and plank, and lately went through the required operation. The particulars were as follow.

The chain B was first measured in five successive removes, the first space of 20 feet having a thermometer in the middle of the bar, which stood at 48°; the second space or remove, having the same thermometer at 48°,2; the third, at 48°,5; the fourth, at 48°,8; and the 5th, at 48°,8; which gave the total length of the chain = 100 feet + 0,077 parts of an inch, in the mean temperature of 48°,6.

The standard A was then measured in five successive removes; the thermometer at each remove being 48°,5 — 48°,6 — 48°,7 — 48°,8 — 48°,8; which gave the length of A = 100 feet + 0,132 parts of an inch, in the temperature 48,°7.

From the Table of expansions in Vol. LXXV. of the Phil. Trans. the difference between the expansion of a rod of steel

and one of cast iron, both of ten feet in length, is found to be 0,00001 part of an inch; therefore, the length of the chains, in the temperature of 54°, agreeing with the points on the bar, will be A = 100 feet + 0,1325 inches.

B = 100 feet + 0,0778 inches.

In the Phil. Trans. for 1795, page 437, their lengths, in the same temperature, as deduced by Mr. RAMSDEN, are stated to be A = 100 feet + 0,11425 inches.

B = 100 feet + 0,05825 inches; which gives a difference something less than $\frac{2}{100}$ of inch between their present and former lengths.

In the reduction of the preceding base, I have supposed the working chain A to be 0,12363 parts of an inch too long before the measurement began. If to this the whole wear be added, *viz.* 0,00384, we shall have the length of it, 100 feet + 0,1275 parts of an inch; which differs only $\frac{5}{1000}$ from the late determination of Mr. BERGE.

Calculation of the Sides of a Series of Triangles, extending from Dunnose, in the Isle of Wight, to Clifton, in Yorkshire. Plate XV.

In the former accounts of the trigonometrical operations it will be found, that triangles have been carried on from Dunnose to Arbury-Hill. It will be proper to give them in this place, that the series may be complete, thereby superceding the necessity of frequently referring to those papers.

Butser Hill from Dunnose, 140580,4 feet. Phil. Trans. for 1795, p. 501.

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
I.	Butser Hill -	76 12 22	−1,99			76 12 21,5	
	Dean Hill - -	48 4 32,25	−1,54			48 4 31,75	
	Dunnose -	55 43 7	−1,53			55 43 6,75	
		180 0 1,25		5,0	−3,75		
	Dunnose from Butser Hill -						140580,4
	Dunnose from Dean Hill - -						183496,2
II.	Dean Hill - -	62 22 48,75	−1,37			62 22 47	
	Butser Hill -	48 28 41,5	−1,23			48 28 40	
	Highclere - -	69 8 35	−1,5			69 8 33	
		180 0 5,25		4,07	+1,18		
	Dean Hill from Butser Hill - -						156122,1
	Dean Hill from Highclere - - -						125084,9
III.	Butser Hill -	84 31 45,5	−1,2			84 31 44,5	
	Hind Head - -	66 15 54,5	−0,83			66 15 54,25	
	Highclere -	29 12 22	−0,72			29 12 21,25	
		180 0 2		2,7	−0,7		
	Butser Hill from Hind Head - -						78905,7
	Butser Hill from Highclere - -						148031,0
IV.	Highclere - -	34 46 15,75	−0,81			34 46 15	
	Hind Head -	83 20 14,25	−1,36			83 20 14	
	Bagshot Heath -	34 46 15,75	−1,88			61 53 31	
		180 0 1,75		3,09	−1,34		
	Highclere from Bagshot Heath - -						142952,6
	Highclere from Hind Head - -						160972,2

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
V.	Bagshot Heath -	55 32 26	—0,89			55 32 25,25	
	Highclere -	46 10 18,25	—0,83			46 10 17,75	
	Nuffield - -	78 17 18,25	—1,20			78 17 17	
		180 0 2,5		2,94	—0,43		
	Nuffield from { Bagshot Heath - -						105321,2
	Nuffield from { Highclere - - -						120374
VI.	White Horse Hill -	63 7 53,25	—0,94			63 7 53,5	
	Highclere - -	63 18 16,75	—0,94			63 18 17	
	Nuffield - -	53 33 49,5	—0,86			53 33 49,5	
		179 59 59,5		2,74	—3,24		
	White Horse Hill from { Nuffield - -						120557 7
	White Horse Hill from { Highclere - -						108503,1
VII.	White Horse Hill -	38 48 13,25	—0,67			38 48 12,5	
	Nuffield - -	86 4 16,25	—1,21			86 4 15	
	Brill - -	55 7 33,5	—0,71			55 7 32,5	
		180 0 3		2,6	+0,4		
	Brill from - { White Horse Hill -						146603,2
	Brill from - { Nuffield - - -						92805,5
VIII.	Brill - -	50 14 44,5	—1,18			50 14 45	
	White Horse Hill -	64 45 43,75	—1,34			64 45 42,5	
	Stow on the Wold -	64 59 32	—1,35			64 59 32,5	
		180 0 0,25		3,88	—3,63		
	Stow from - { White Horse Hill -						124365,6
	Stow from - { Brill - - -						146326,3
IX.	Brill - -	32 34 43	—0,61			32 34 42,25	
	Stow on the Wold -	60 56 6,25	—0,64			60 56 5,5	
	Epwell - -	86 29 13,25	—0,11			86 29 12,25	
		180 0 2,75		2,37	+0,38		
	Epwell from - { Stow - - -						78938,2
	Epwell from - { Brill - - -						128140

No. of triangles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
X.	Brill - -	34 23 58,5	−0,65			34 23 57,5	
	Epwell - -	85 0 18,5	−1,10			85 0 17,5	
	Arbury Hill -	60 35 45,5	−0,70			60 35 45	
		180 0 22,5		2,46	+0,04		
	Arbury Hill from { Epwell - - -						83098,4
	Arbury Hill from { Brill - - -						146530
XI.	Arbury Hill -	89 57 4,5	−1,14			89 57 5,5	
	Epwell - -	54 45 18,75	−0,57			54 45 18,25	
	Corley - -	35 17 36,75	−0,57			35 17 36,25	
		180 0 0		2,29	−2,29		
	Corley from - { Arbury Hill - -						117463
	Corley from - { Epwell - - -						143827,8

By the last triangle, the distance from Corley to Arbury Hill is 117463 feet, which distance, and all the others constituting the sides of this part of the series, are deduced from the base on Hounslow Heath, as well as that on Salisbury Plain. With regard to the triangles connecting the stations at Corley and Arbury Hill with the base recently measured in the north, it will be proper to let them rest partly on that base, and partly on the side Corley and Arbury Hill. And here I would remark, that in carrying on a series of triangles, whether for the purpose of a meridional measurement or otherwise, it is proper that a base of verification, answering at the same time as a new one of departure, should be measured every hundred miles at least. With this idea, therefore, the foregoing triangles, as well as those composing the remaining part of the series, should be furnished with three base lines, *viz.* one at each extremity, and the other in the middle. In calculating the sides, were the series thus

circumstanced, it would be right to depend on each base for one third of the distance between it and the one next at hand, and use the mean result, as derived from the two adjoining bases, for the true lengths of the several sides within the other third. Thus, if two bases were found at the extremities of the arc in question, and one in the middle, as about Brill, the computation should be carried on, from the extreme bases, about one-sixth part of the meridional distance; and, from the middle base, one-third of the intermediate distance on each side; the remaining two arcs being determined from the respective base lines. That I may avoid prolixity, or the appearance of it, I shall compute the sides of the triangles northward of the two stations before mentioned, from the base measured on Misterton Carr only, and use the mean distances calculated on the above principle, when I find the total length of my arc.

Length of the Base on Misterton Carr, 26342,7 Feet.

No. of angles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error.	Angles corrected for calculation.	Distance
		° ′ ″	″	″	″	° ′ ″	Feet.
	Beacon Hill -	20 47 19,75				20 47 20	
XII.	North end of Base	60 17 16,5				60 17 13	
	South end of Base	98 55 27,5				98 55 27	
		180 0 3,75					
	Beacon Hill from { North end of Base -						64461
	Beacon Hill from { South end of Base -						73321,
	Beacon Hill -	34 44 42,25				34 44 42	
XIII.	North end of Base	74 46 56,5				74 46 56	
	Gringley on the Hill	70 28 22,25				70 28 22	
		180 0 1					
	Gringley from { North end of Base -						44338
	Gringley from { Beacon Hill -						75068

No. of Triangles.	Names of stations.	Observed angles.	Diff.	Spheri-cal excess.	Error.	Angles corrected for calculation.	Dista
		° ′ ″	″	″	″	° ′ ″	Fe
	Beacon Hill - -	13 57 24				13 57 23	
XIV.	Gringley - -	51 11 6,5				51 11 5	
	South end of Base	114 51 32,5				114 51 32	
		180 0 3					
	Gringley from Beacon Hill - -						750
	Wherefore the mean distance from Gringley to Beacon Hill is 75068,1 Feet.						
	Heathersedge -	18 40 38,5	+0,29			18 40 38	
XV.	Beacon Hill -	138 9 16	−2,02			138 9 16	
	Gringley - -	23 10 6	+0,65			23 10 6	
		180 0 0,5		1,08	−0,58		
	Heathersedge from { Beacon Hill - -						922
	Gringley - - -						1563
	Sutton Ashfield -	78 47 2	−1,01			78 47 1	
XVI.	Heathersedge -	54 52 37,5	−0,24			54 52 35	
	Gringley - -	46 20 24	−0,22			46 20 24	
		180 0 3,5		2,45	+1,10		
	Sutton Ashfield from { Gringley - - -						1303
	Heathersedge - -						1153
	Orpit - -	80 28 57,25	−0,85			80 28 57	
XVII.	Heathersedge -	39 8 38,5	−0,12			39 8 38	
	Sutton Ashfield -	60 22 25,5	−1,00			60 22 25	
		180 0 1		2,03	−1,03		
	Orpit from { Heathersedge - -						1011
	Sutton Ashfield - -						73

of ngles.	Names of stations.	Observed angles.	Diff.	Spherical excess.	Error	Angles corrected for calculation.	Distances.
		° ′ ″	″	″	″	° ′ ″	Feet.
III.	Hollan Hill -	44 43 32	−0,12			44 43 31	
	Sutton Ashfield -	113 49 9	−0,53			113 49 7	
	Orpit - -	21 27 20,5	−0,18			21 27 22	
		180 0 1,5		0,73	+0,77		
	Hollan Hill from { Sutton Ashfield - -						38375,2
	Hollan Hill from { Orpit - - - -						95975,3
IX.	Bardon Hill - -	42 58 59,5	−0,69			42 58 59	
	Hollan Hill -	74 52 38	−1,03			74 52 37	
	Orpit - -	62 8 25	−1,01			62 8 24	
		180 0 2,5		2,75	−0,20		
	Bardon Hill from { Hollan Hill - - -						124454,7
	Bardon Hill from { Orpit - - -						135895,3
X.	Castle Ring -	55 32 44	−0,94			55 32 43	
	Bardon Hill -	68 24 4,75	−1,02			68 24 3	
	Orpit - -	56 3 14,75	−0,90			56 3 14	
		180 0 3,5		2,85	+0,65		
	Castle Ring from { Orpit - - -						153235,
	Castle Ring from { Bardon Hill - - -						136717,
XI.	Corley -	72 32 46,5	−1,19			72 32 46	
	Castle Ring -	47 54 42,25	−0,86			47 54 42	
	Bardon Hill -	59 32 32,25	−0,94			59 32 32	
		180 0 1		2,93	−1,93		
	Corley from - { Bardon Hill - -						106357,
	Corley from - { Castle Ring - -						123539,
XII.	Arbury Hill -	34 14 33,5	−0,98			34 14 33	
	Corley -	107 20 14,25	−1,99			107 20 14	
	Bardon Hill -	38 25 13,25	−0,80			38 25 13	
		180 0 1		3,37	−2,37		
	Arbury Hill from { Bardon Hill - -						180426,
	Arbury Hill from { Corley - - - -						117457,

From the last triangle, we get 117457,1 for the distance between Corley and Arbury Hill. By the XI. triangle, the distance between those stations is 117463 feet; there is, therefore, a difference of nearly six feet between the two determinations; a quantity which cannot be considered unexpectedly great, as the side is more than twenty-two miles in length, and the whole series nearly two hundred miles long. If the computation had been carried on from Dunnose all the way up, the bases on Hounslow Heath and Salisbury Plain would have given the length of that on Misterton Carr about one foot greater than its measured extent. If the sides of the triangles contiguous to Corley and Arbury Hill be recomputed, from the mean distance between those stations, *viz.* 117460 feet, no doubt whatever can be justly entertained of the general accuracy of the whole. These mean distances, as I have before observed, will be used in the calculations of the total length of the meridional arc. From the Base in the north, I have numbered the triangles downwards: the reason is obvious.

Calculation of the meridional Distance between Dunnose and Clifton.

To do this, it will be right to compare the distances of the several stations from the respective perpendiculars, both of Dunnose and Clifton, as derived from the observed direction of each meridian.

In the Phil. Trans. for 1795 it will be seen, that the direction of the meridian was observed at the station on Dunnose, in 1793, the staff to which the pole star was referred being placed on Brading Down. The angle between that staff and the meridian, (see page 517 of that volume,) was found to be 21° 14′ 11″,5, as

derived from two double azimuths of the star, supported by several computed azimuths, applied to single but accurate observations.

The angle between Butser Hill and the staff at Brading Down was 0° 15′ 35″,5. This, with the above angle, 21° 14′ 11″,5, and particular angles of the series, gives,

The Bearings of certain Sides from the Parallels to the Meridian of Dunnose.

Dunnose and Butser Hill	-	20°	58′	39″	NE
Butser Hill and Highclere	-	34	20	17	NW
Highclere and Nuffield	- -	35	30	40	NE
Nuffield and Brill	- -	4	51	15	NW
Brill and Arbury Hill	- -	12	30	17	NW
Arbury Hill and Bardon Hill	-	7	42	57	NW
Bardon Hill and Orpit	- -	21	21	9	NW
Orpit and Heathersedge	-	5	25	52	NW
Heathersedge and Beacon Hill	-	61	52	17	NE.

These bearings, and the respective sides, give the following distances on the meridian of Dunnose, *viz.*

		Feet.	Miles.
Dunnose and Butser Hill	-	131263,0 =	24,86
Butser Hill and Highclere		122232,7 =	23,15
Highclere and Nuffield	-	97984,7 =	18,56
Nuffield and Brill	- -	91755,3 =	17,38
Brill and Arbury Hill	-	143054,1 =	27,09
Arbury Hill and Bardon Hill		178792,4 =	33,86
Bardon Hill and Orpit	-	126567,8 =	23,97
Orpit and Heathersedge	-	101203,7 =	19,17
Heathersedge and Beacon Hill		43480,7 =	8,23
		1036334,4 =	196,27, the distance

between Clifton and the perpendicular to the meridian of Dunnose; which may be taken for the true length of the arc itself, as the distance of the former station from the meridian of the latter, is only 4770 feet.

If the angle between the meridian and the staff at Brading Down was observed accurately, there can be no doubt of the correctness of this determination; but, as it was right on my part to adopt measures for bringing it to some proper test, I observed, as before stated, the direction of the meridian at Clifton. The particulars were as follows.

Observed Angles between the Pole Star, when at its greatest Elongations from the Meridian of Clifton, and the Staff erected over the Station at Gringley on the Hill.

August, 1801.

Days.		Evenings.	Mornings.
9th.	-	100° 45′ 46″	
10th.	-	100 45 43,5	
11th.	-	100 45 45,5	106° 39′ 34″
13th.	-	100 45 39	106 39 22
16th.	-	100 45 40,5	
17th.	-	100 45 41	106 39 24
18th.	-	100 45 39	106 39 28
19th.	-	100 45 46,5	106 39 27.

If a mean of all the evening observations be taken, we shall get 100° 45′ 42″,8, for the angle between the staff at Gringley and the star when at its greatest eastern elongation from the merdian. In like manner, if a mean of all the morning observations be taken, we shall have 106° 39′ 27″, for the angle between the same staff and the star on the western side. Hence, half their

sum, 103° 42′ 35″, nearly, will be the angle between Gringley and the meridian of Clifton; and its south-eastern bearing 76° 17′ 25″. This, with certain angles of the series, gives the bearings of the following sides, *viz.*

Beacon Hill and Heathersedge -	61°	51′	50″	SW
Heathersedge and Orpit - -	5	26	19	SE
Orpit and Bardon Hill - -	21	21	36	SE
Bardon Hill and Arbury Hill -	7	43	26	SE
Arbury Hill and Brill - -	12	31	0	SE
Brill and White Horse Hill -	50	15	48	SW
White Horse Hill and Highclere	27	48	6	SE
Highclere and Butser Hill - -	34	20	49	SE
Butser Hill and Dunnose -	20	58	9	SW.

These bearings and sides give the following parallels to the meridian of Clifton.

	Feet.
Beacon Hill and Heathersedge	43490,4
Heathersedge and Orpit -	101202,6
Orpit and Bardon - -	126561,3
Bardon Hill and Arbury Hill -	178793,2
Arbury Hill and Brill -	143047,4
Brill and White Horse Hill -	93717,6
White Horse Hill and Highclere	96031,4
Highclere and Butser Hill -	122219,8
Butser Hill and Dunnose -	131270,2

The sum, 1036333,9 feet, is the distance between Dunnose and the perpendicular to the meridian of Clifton; or, as observed with regard to the sum of the parallels to the meridian of the former, the length of the arc itself.

There is, therefore, a difference of only half a foot, between the two results. We may, consequently, take 1036334 feet, for the distance required.

I have observed, in a former part of this account, that the zenith sector was placed $6\frac{1}{2}$ feet from the station at Dunnose, and $3\frac{1}{2}$ feet from that at Clifton, the new points being due south of the old. We must therefore add 3 feet to 1036334; which gives 1036337 feet, for the total length of the arc of the meridian.

The sum of the parallels to the meridian of Clifton, reaching down to Arbury Hill, is 450047,5 feet; and the distance of the latter from that meridian 1996 feet. This is, in fact, the meridional extent between the two old stations, as no correction is requisite. We must, however, subtract 30 feet from this distance, as the sector was put up $34\frac{1}{2}$ feet northward of the station on Arbury Hill. Therefore, 450047,5 — 30 = 450017,5 feet, is the length of the arc comprized between the parallels of the new stations at Clifton and Arbury Hill: and, subtracting this from 1036337, we have 586319,5 feet, for the distance of this latter station from the point over which the sector was placed at Dunnose.

Although the zenith sector was taken to the Royal Observatory at Greenwich, rather with a view of collecting materials for finding the latitude of Dunnose, than to answer any other purpose, yet, as I am provided with the means of finding the meridional distance between those places, and that with sufficient accuracy, I shall go through the work in this place.

Distance between the Parallels of Latitude of Greenwich and Dunnose.

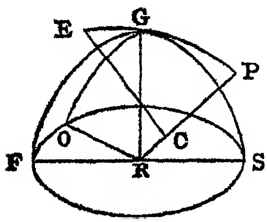
In the Phil. Trans. for 1795, the station on Beachy Head is shown to be 269328 feet from the perpendicular at Greenwich, and 58548 from its meridian. In Plate XVI. Fig. 2, of this account, let DPB be a great spheroidical triangle on the earth's surface, P the pole, and D B the two stations at Dunnose and Beachy Head. Let also PGM be the meridian of Greenwich, [G,] and M the point where the parallel of Beachy Head to the perpendicular at G cuts that meridian. Then, from the above values of GM and BM, it will be found, that the latitude of B is 1″,03 less than the latitude of M, and that too on any hypothesis of the earth's figure. Therefore, the distance in feet, between the parallels of B and G, is 269328 + 103 = 269431.

Now it has been shown, in the volume above referred to, (see page 522,) that the meridional distance between D and B is the mean of the two numbers 44258,6 and 44258,9 feet; and it must be remembered that, in deducing those conclusions, recourse was not had to matters of assumption, but to matters of fact, which were, the observed directions of the two meridians PD, PB, and the distance DB. Therefore, if 44259 feet be taken for the meridional distance between D and B, we shall have 269431 + 44259 = 313690 feet, for the space between the parallels of latitude of Greenwich and Dunnose.*

* In the Phil. Trans. for 1800, (see note to page 641,) in finding the value of the oblique arc between Black Down, in Dorsetshire, and Dunnose, I have used the expression $\frac{pm}{p+\overline{m-s}\,.\,s^2} = d$; where d is the length of the required degree, p that of the great circle perpendicular to the meridian, m that of the degree of the meridian itself, and s the sine of the angle constituted by the oblique arc and the meridian.

We have then found the total length of the whole meridional arc, and also the distances of two intermediate points from either, or from both, of its extremities. And, to bring the whole

The demonstration of this rule I did not insert, on account of the previous length of the note alluded to; but I take this opportunity of supplying the omission, in the words of Mr. DALBY.



Having the length of the degree on the meridian, and also that of the degree perpendicular to it, at the same point; to find the length of a degree in any other given direction, supposing the earth to be an ellipsoid.

Let EP be one-fourth of the elliptic meridian; C the centre of the earth; CE, CP, the equatorial and polar semiaxes; G a given point on the meridian EP. Draw GR perpendicular to the meridian at G, meeting the axis PR in R; then RG is the radius of curvature of the ellipse, at the point G, which is perpendicular to the meridian at G.

Conceive another ellipsoid FGSO to touch the given one in the point G. Then, it is evident, that if the curvature be respectively the same in the direction of the meridian and the perpendicular, on both ellipsoids at the point G, the curvature will also be equal on both figures, in any other direction at that point. And the like is manifest in spheroids of any other kind.

Let M be the radius of curvature of the meridian at the point G; then, because RG is the radius of curvature in the perpendicular direction, if we take FS (at right angles to RG) $= 2\sqrt{RG \times M}$, and about FS, the axis to the semidiameter RG, describe the ellipsoid FGSO, it will be *that* having the curvature of G the same as on the other ellipsoid at that point.

Let OGR be the plane of an ellipse, inclined to the meridian EGP, or to the plane FGS, in a given angle FRO, whose *sine* and *cosine* are s and c. Then, since RG, or rather its equal, is a semitransverse, in the plane FOSR, (which is perpendicular to RG,) to the semiconjugate RF, we shall have $\frac{RG^2 \times RF^2}{RG^2 c^2 + RF^2 s^2} = RO$, which, divided by RG, (RG being the semitransverse to RO in the perpendicular plane ROG,) gives $\frac{RG \times RF^2}{RG^2 c^2 + RF^2 s^2}$ for the radius of curvature of the inclined ellipse OG at the point G. But, because the lengths of the degrees are proportional to their radii of curvature, if we put m and p for the meridional and perpendicular degrees, then RF or $\sqrt{RG \times M}$ and RG may be expounded by $\sqrt{pm}$, and p; hence, the expression will become $\frac{pm}{pc^2 + ms^2}$, for the length

under one point of view for future use, we shall have the following

Arcs.

		Feet.		Miles.
1.	Clifton and Dunnose -	1036337	=	196,27
2.	Dunnose and Arbury Hill -	586320	=	111,05
3.	Dunnose and Greenwich - -	313696	=	59,41
4.	Clifton and Arbury Hill -	450017	=	85,23
5.	Clifton and Greenwich - -	722641	=	136,86
6.	Arbury Hill and Greenwich -	272624	=	51,63

Remark.

In calculating the distance between the parallels of latitude of two places, connected by means of a trigonometrical operation, regard must be had to their difference in longitude. If the triangles run nearly north and south, in which case stations must lie both east and west of the two meridians, it is sufficiently correct to proceed on the supposition of the earth's surface being a plane; but if, on the contrary, the triangles wholly diverge from the two meridians, or even partly do so, first running off obliquely and then returning again, a different

of the degree oblique to the meridian; or, putting $1 - s^2$ for c^2, and r for $p - m$, it will be $\frac{pm}{p - rs^2}$.

Corol. If d be the length of the oblique degree, then, since $d = \frac{pm}{pc^2 + ms^2}$, we have $p = \frac{s^2 dm}{m - c^2 d}$, and $m = \frac{c^2 dp}{p - s^2 d}$. And, if D be put for the length of another oblique degree at the same point, and S and C the *sine* and *cosine* of its inclination to the meridian, we shall get $m = \frac{S^2 c^2 - C^2 s^2}{S^2 D - s^2 d} \times Dd$, and $p = \frac{S^2 c^2 - C^2 s^2}{c^2 d - C^2 D} \times Dd$, the meridional and perpendicular degrees, exhibited in terms of the oblique degrees combined with the sines and cosines of their inclinations to the meridian. Therefore, an ellipsoid may be determined from the lengths of two oblique degrees in the same latitude.

We may likewise remark, from the nature of radii of curvature, at the same point G, that the expression $\frac{pm}{p - rs^2}$ will also give the oblique degree on different spheroids.

method must be pursued. The necessity giving rise to this, originates from the radii of curvature of the oblique degrees continually varying, and the angles of convergency, between the several sides and their respective meridians, remaining unknown.

It must be remembered, that the sides of the several triangles projected over the country, in this Survey, are not to be considered as so many distances on the earth's surface, but the lengths of the chord lines subtended by arcs. Therefore, it is manifest that, strictly speaking, all the chord angles should be used, and not the horizontal ones; with which, after the bearing of the first side with the meridian has been reduced to some plane beneath the earth's surface, a number of chord lines in the plane of that meridian are to be computed; the sum of which, augmented by the differences between those chords and their respective arcs, will give the true meridional distance. I have been at the trouble to calculate the distance between Clifton and Dunnose on this principle; and find the length of my arc to be 1036339,5 feet; which is, about $2\frac{1}{2}$ feet more than the distance determined by the other mode of computation. An advantage, however, attending a calculation on the principle now spoken of, is the ability of calculating, pretty nearly, the azimuth of any one station from an extremity of the arc. This, if the instrument with which the direction of the meridian is observed be not well divided, or otherwise not exactly fit for the operation, is necessary, and should be always done. The angle at Clifton, between Gringley on the Hill and the meridian, was observed to be 76° 17′ 25″. According to my computation in the way spoken of, that angle is 76 17′ 30″. A difference of 5″, working all the way up from Dunnose through an arc of 2° 50′, is as small as can be expected, and serves to prove that the angles of

the triangles, as well as the observed direction of the meridians, are consistent. I have given the meridional distance between Clifton and Dunnose, bearings of the sides, &c. deduced from the most simple of the two methods; first, because the result is sufficiently accurate; secondly, because it places within general reach, the means of examining this part of my operation. In attending to this remark, it must be remembered, that a line from Dunnose perpendicular to the meridian of Clifton, is only 4853 feet.

SECTION SECOND.

Operations at the Station on Dunnose, the Southern Extremity of the Arc, with the Zenith Sector. May and June, 1802.

On the 8th of May, the circular or large theodolite was placed over the point selected for a new station: its distance was $6\frac{1}{2}$ feet from the gun, and in a direction due south. The following objects were then observed, the readings of which, on the graduated limb, were as follows.

Sir R. Worsley's obelisk (the top) -	113°	14′	28″
East Cowes sea mark - - -	1	46	36,5
Luttrell's Folly - - -	177	56	25
Vane on the top of Portsmouth Church	40	6	44,5
Sir R. Worsley's obelisk, a second time	113	14	24,25

The above objects were observed, in order that no possible mistake might result; as (though not probable) accidental circumstances might have given rise to a wrong statement of the bearing of some one of the number, (except Portsmouth Church,) in the account of 1795. Omitting the obelisk, the

bearings of the other objects, as extracted from that Paper, will be as follows.

LUTTRELL'S Folly	23°	0′	44″	NW	from the meridian of Dunnose.
East Cowes sea mark	19	11	19	NW	
Portsmouth Church	19	9	40	NE	

If, from the readings on the limb, the angles between the obelisk and the other objects be taken, and applied to the last-mentioned bearings, we shall get the angle between the obelisk and the meridian, 87° 42′ 40″
35
45 } Mean, 87° 42′ 40″.

May 9th. Erected the observatory, drove four long stakes into the ground, and brought their several heads into the same horizontal plane. Then erected the stand, set up the sector, and adjusted the axis level, and the axis itself; determined the exact weight the plumb-line would bear, and then examined how much the cross wires were out of their proper positions, as follows.

The stand being firmly screwed down to the stakes, the sector was turned on its axis, till the pointed top of Sir RICHARD WORSLEY'S obelisk appeared in the field; it was then clamped to the azimuth circle, but subject to a small motion by turning an adjusting-screw. The pointed apex was then made to appear as just vanishing under the wires; in which situation of things, the side telescope was turned round, and laid in its several positions on the brass frame attached for its reception to the side of the sectorial tube; the top of the obelisk appearing as a vanishing point under the wires. On whichever face of its squares it was made to rest, the vernier of the azimuth circle read off to 84° 5′. The little telescope was then taken out of its frame, and the

sector turned half round. It was then again introduced into its supports, and the interior stand moved, till the wires in the focus of the lateral telescope appeared on the obelisk as before. The vernier was then examined, which again stood at 84° 5′. This being settled, the sector was turned round, till its vernier stood at 176° 22′ on the azimuth circle, in which situation, the plane of the divided arc was necessarily parallel to that of the meridian. The task of observation then commenced, and the performance of it was as follows.

Observations made at Dunnose, to determine the Zenith Distance of β Draconis.

Point on the Limb, 1° 50′ North.

Day of the month.	Face of the arch, W. or E.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
1802		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
May 11	W	9 4,82	9 17,9	1 50 0 13,08	1 49 46,90	28,85	43,5	43,5
13	E	9 16,95	8 56,0	19,95	40,02	28,85	36,5	38,0
14	W	9 34,25	9 47,5	13,25	46,73	28,92	34,5	34,5
16	E	8 32,16	8 14,0	18,16	41,81	28,82	35,5	34,5
June 5	W	6 23,00	6 30,0	7,00	52,99	28,45	51,5	51,5
8	E	8 14,02	8 2,0	12,02	47,96	28,49	52,0	51,8
11	W	6 57,40	7 2,6	4,20	55,79	28,54	52,5	52,0
13	E	9 39,50	9 29,5	10,00	49,98	28,79	53,0	52,7
14	W	8 19,29	8 23,7	4,41	55,58	28,86	54,2	53,0
16	E	3 56,61	3 47,0	9,61	50,37	28,75	59,5	60,0
17	W	8 38,52	8 41,5	2,98	57,02	28,82	56,1	58,0
18	E	11 31,87	11 21,5	10,37	49,61	28,81	52,0	51,0
20	W	8 53,27	8 54,2	0,93	59,07	29,03	57,5	58,0
21	E	10 27,05	10 19,7	7,35	52,64	28,99	56,5	55,5

Observed Zenith Distances of γ Draconis.

Point on the Limb, 0° 50′ North.

Day of the month.	Face of the arch, W. or E.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts	Zenith distance reduced.	Barometer.	Thermometer Above.	Thermometer Below
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches	°	°
May 10	E	10 15,52	13 48,1	0 50 3 32,75	0 53 30,10	29,0	—	45,
11	W	9 38,66	5 56,4	41,26	38,62	28 85	43,9	43,
13	E	8 47,30	12 81,4	34,10	31,45	28,85	36 5	38,
14	W	7 32,38	3 49,2	42,18	39,54	28,92	34,5	34,
16	E	9 40,00	13 15,2	34,20	31,55	28,82	35,5	30,
June 11	W	7 20,70	3 29,5	50,20	47,58	28,34	53,5	52,
13	E	9 36,35	13 20,3	42,95	40,31	28,79	52,5	52,
14	W	8 25,26	4 33,4	50,86	48,24	28,26	54,3	53,
16	E	9 48,33	14 37,4	45,07	43,44	28,75	59,5	60,
17	W	8 32,66	4 39,4	52,26	49,64	28,82	56,0	58,
18	E	11 32,77	15 17,9	44,13	41,50	28,8	52,0	51,
20	W	8 9,48	4 17,0	51,48	48,86	29,97	58,6	57,
21	E	11 52,92	15 40,0	47,08	44,45	28,83	56,0	55,

Observed Zenith Distance of 45 d Draconis.

Point on the Limb, 6° 15′ North.

June 13	E	9 27,76	10 53,1	6 15 1 25,34	6 16 24,48	28,8	49,5	51,
14	W	9 23,81	7 48,5	34,31	33,30	28,86	54,0	53,
16	E	10 18,90	11 46,5	27,60	26,74	28,77	59,0	59,
18	W	11 11,65	9 32,9	37,75	36,91	28,8	58,5	52,
20	E	8 18,20	9 46,7	28,50	27,64	28,97	56,0	55,
21	W	11 31,03	9 52,7	37,33	36,49	28,99	56,0	55,

Observed Zenith Distance of 46 c Draconis.

Point on the Limb, 4° 40′ North.

June 13	E	9 34,85	12 46,0	4 40 3 11,15	4 43 8,46	28,8	49,5	51,
14	W	8 16,17	4 53,0	20,17	17,50	28,86	51,7	50,
16	E	13 15,62	16 27,0	11,38	8,69	28,77	59,0	59,
18	W	11 10,40	7 41,3	18,10	15,42	28,8	52,7	50,
20	E	8 32,10	11 47,6	14,90	12,22	28,97	58,4	57,
21	W	11 5,13	7 43,1	21,03	18,36	28,99	56,0	55,

Observed Zenith Distance of 51 *Draconis.*
Point on the Limb, 2° 25′ *North.*

Day of the month	Face of the arch, W. or E.	Plumb-line.	Observation of the star	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above	Thermometer. Below.
		rev. div.	rev. div	° ′ rev. div.	° ′ ″	Inches	°	°
June 13	E	7 42,00	13 11,5	2 20 3 28,50	2 23 25,84	28,8	49,5	51,0
14	W	8 10,63	4 33,3	36,33	33,68	28,9	51,8	50,5
16	E	9 0,00	12 28,8	28,80	26,14	28,8	59,0	59,5
18	W	10 48,27	7 11,5	36,77	34,12	28,8	50,2	50,0
20	E	8 47,43	12 21,0	32,57	29,92	29,0	55,0	56,0
21	W	11 2,23	7 24,5	36,75	34,08	29,0	53,5	55,0

Observed Zenith Distance of μ *Draconis.*
Point on the Limb, 4° 5′ *North.*

May 11	W	9 18,20	7 31,6	4 5 1 45,60	4 6 44,73	28,85	43,5	43,5
13	E	13 3,04	11 39,0	46,01	35,17	28,85	40,5	41,0
14	W	10 2,03	8 18,4	42,63	41,80	28 92	36,3	38,5
June 8	E	8 12,56	9 57,1	44,54	43,71	28,51	51,5	51,0
13	E	9 29,34	11 15,8	45,46	44,63	28,79	53,5	51,5
14	W	8 29,56	6 34,7	53,86	53,05	28,86	53,5	54,0
16	E	4 9,60	5 56,0	46,34	45,51	28,75	59,5	60,0
17	W	8 45,33	6 50,5	53,83	53,02	28,82	56,0	58,0
18	E	11 44,03	13 34,0	48,17	47,35	28,80	52,0	51,1
20	W	8 44,96	6 47,0	54,96	54,15	29,0	58,2	58,0
21	E	10 34,60	12 25,0	49,40	48,58	28,99	55,8	55,5

Observed Zenith Distance of 16 *Draconis.*
Point on the Limb, 2° 40′ *North.*

May 11	W	10 2,08	7 43,0	2 40 2 18,08	2 42 16,30	28,85	43,5	43,5
14	W	10 27,15	8 9,0	18,15	16,37	29,92	36,5	37,2
16	W	8 31,87	6 14,0	17,87	16,09	28,82	39,0	39,9
June 5	W	9 38,25	7 11,5	26,75	24,99	28,54	53,5	52,0
13	E	4 28 90	1 48,3	39,60	37,86	28,86	52,0	51,5
14	W	8 31,63	6 4,0	27,63	25,87	28,86	53,5	54,0
16	E	3 51,90	6 14,5	21,60	19,83	28,78	61,0	60,2
18	E	11 28,70	13 50,1	21,40	19,63	28,80	51,5	52,7
20	W	8 25,61	5 55,2	29,41	27,65	28,95	57,7	58,5

Observed Zenith Distance of 1 κ Cygni.
Point on the Limb, 2° 20′ North.

Day of the month.	Face of the arch, W. or E.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer	Thermometer. Above	Thermometer. Below.
		rev div.	rev. div	° ′ rev. d v.	° ′ ″	Inches.	°	°
June 13	E	9 26,60	12 33,5	2 20 3 6,90	2 23 4,20	28,8	50,0	50,5
14	W	5 54,98	2 37,0	17,98	15,30	28,9	52,1	50,0
16	E	8 49,45	6 52,9	1 55,55	5,26	28,8	59,9	59,0 *
18	W	10 12,05	6 58,8	2 20 3 16,25	13,57	28,8	50,1	50,0
20	E	9 9,83	12 20,0	10,17	7,48	29,0	54,9	56,1 *
21	W	11 1,83	7 44,2	16,63	13,95	29,0	55,0	53,5

* Point on the limb 2° 25′.

Observed Zenith Distance of 10 ι Cygni. ‡
Point on the Limb, 0° 40′ North.

June 13	E	9 30,95	10 55,2	0 40 1 24,25	0 41 23,39	28,8	50,0	50,5
14	W	6 50,05	4 51,8	32,25	31,40	28,9	51,7	50,5
16	E	8 43,00	10 9,5	25,50	24,64	28,8	60,0	59,0
18	W	9 38,90	8 26,0	32,90	32,05	28,8	50,2	50,0
20	E	8 30,78	10 0,0	28,22	27,36	29,1	55,0	56,0
21	W	11 19,80	9 45,1	33,70	32,85	28,9	55,0	55,5

Observed Zenith Distance of γ Ursæ.
Point on the Limb, 4° 10′ North.

May 9	E	11 42,53	12 14,6	4 10 0 31,07	4 10 31,12	—	—	—
11	E	11 10,60	11 42,5	31,90	31,95	29,0	52,0	52,0
11	W	8 34,32	7 54,4	38,92	38,98	28,8	50,5	51,5
13	E	9 54,86	10 29,5	33,64	33,69	28,8	45,7	44,3
14	E	10 1,47	10 34,5	33,03	33,08	28,9	38,5	38,5
15	W	7 15,49	6 33,6	40,89	40,96	28,9	41,0	41,5
17	E	8 54,00	9 29,5	34,50	34,56	28,8	46,0	42,5
20	E	13 43,03	14 18,1	34,07	34,13	28,7	50,5	51,0
June 5	W	9 21,10	8 35,5	43,60	43,67	28,4	53,0	55,1
8	E	8 58,90	9 36,2	37,30	37,36	28,5	55,5	58,5
12	E	5 46,50	6 22,4	34,90	34,96	28,6	54,0	54,0 †
13	W	10 40,7[illegible]	9 56,4	43,30	43,37	28,7	59,1	59,0
14	W	7 16,40	6 53,0	42,40	42,47	28,9	60,4	59,3
16	E	9 1,20	9 41,1	38,00	38,06	28,8	72,0	69,5

† Imperfect observations.

‡ In page 417, 10 ν Cygni should be 10 ι, ξ Ursæ should be ζ, 85 ν Herculis should be 85 ι, and φ Herculis should be υ.

Observed Zenith Distance of η Ursæ.
Point on the Limb, 0° 15′ South.

Day of the month.		Face of the arch, W. or E	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
			rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
May	10	E	10 6,74	6 8,6	0 15 3 57,14	5 18 54,53	29,0	49,1	49,0
	13	E	9 48,22	5 49,1	58,12	55,51	28,9	40,5	41,0
	14	W	10 47, 4	14 39,6	5[illegible],86	48,24	28,9	36,5	38,5
	15	E	10 9,99	6 11,1	57,89	55,25	28,9	41,0	41,5
	16	W	9 13,31	13 5,0	50,69	48,07	28,8	39,0	39,9
	17	E	9 0,46	5 3,2	56,26	53,65	28,8	46,0	42,5
June	5	W	7 58,15	11 46,3	47,1[illegible]	44,52	28,5	52,3	52,3
	8	E	9 6,37	5 11,6	53,72	51,10	28,5	52,0	56,0
	11	W	9 28,50	13 12,7	43,20	40,57	28,5	52,0	55,5
	12	E	5 32,60	1 59,0	52,60	49,98	28,6	57,5	56,0
	13	W	10 14,50	14 4,0	48,50	49,87	28,7	56,5	56,5
	14	E	7 47,52	4 0,4	47,12	44,49	28,8	57,0	56,5 •
	16	W	9 4,21	12 49,7	45,49	42,86	28,8	64,0	63,5
	18	W	9 14,50	13 0,5	45,00	42,37	28,8	50,5	57,5
	20	E	9 6,03	5 13,9	51,13	48,41	28,8	67,0	70,5
	21	E	12 45,00	8 57,5	50,50	47,87	28,9	55,5	56,•

• Doubtful.

Observed Zenith Distance of ζ Ursæ.
Point on the Limb, 5° 20′ North.

May	11	W	9 19,98	8 44,3	5 20 0 34,68	50 20 34,74	28,8	48,9	49,5
	13	E	9 37,00	10 5,5	27,50	27,54	28,8	45,1	44,1
	17	E	8 49,96	9 20,5	29,54	29 59	28 8	46,0	42,3
June	5	W	8 31 92	7 51,9	39,02	39,08	28,5	52,3	54,3
	8	E	8 56,50	9 28,1	30,60	30,65	28,5	52,0	56,0
	11	W	9 36,93	8 55,6	40,33	40,00	28,5	54,0	55,0
	14	E	7 47,74	8 23,0	34,26	34,32	28,5	60,5	59,5
	17	E	9 33,12	10 8,2	34,08	34,14	28,8	64,0	65,0
	18	W	9 28,30	8 45,0	42,30	42,37	28,7	57,5	59,5
	20	E	8 54,12	9 30,2	35,08	35,14	28,8	67,0	70,5

Observed Zenith Distance of 85 ι Herculis.
Point on the Limb, 4° 25′ South.

May	10	E	15 16,87	9 50,6	4 25 5 25,27	4 30 20,80	29,0	45,5	45,5
	13	E	8 28,70	3 2,5	26,20	21,73	28,8	40,5	41,0
	14	W	8 24,16	13 43,0	18,84	14,36	28,9	34,5	34,5
	16	E	9 36,40	4 11,2	25,20	20,23	28,8	35,5	35,5
June	14	W	8 25,36	13 33,6	8,24	3,74	28,8	54,0	53,9

Observed Zenith Distance of υ Herculis.

Point on the Limb, 4° 0′ South.

ay of the onth.	Face of the arch, W. or E.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
[ay 11	W	9 25,06	10 55,0	4 0 1 29,94	4 1 29,09	28,8	43,5	43,5 *
13	E	10 3,86	8 12,2	50,66	49,84	28,8	40,5	41,0
14	W	9 44,22	11 27,6	42,38	41,55	28,9	34,5	35,5
16	W	2 56,33	4 40,3	42,97	42,14	22,8	39,0	39,9
ine 5	W	6 25,10	8 3,6	37,50	36,66	28,4	50,5	51,0
11	W	9 26,10	11 21,0	53,90	53,09	28,5	53,5	52,0
13	E	9 58,25	8 17,0	41,25	40,42	28,8	52,0	51,5
14	W	9 27,50	11 3,5	35,00	34,16	28,7	54,7	54,5
16	E	3 53,47	2 13,0	40,47	39,63	28,8	61,0	60,2
18	E	11 42,00	13 23,0	40,00	39,17	28,8	51,5	52,7
20	E	9 34,90	7 53,4	40,50	39,66	28,9	57,7	52,5

* Imperfect observation.

Observed Zenith Distance of 52 Herculis.

Point on the Limb, 4° 15′ South.

[ay 13	E	9 52,55	7 32,0	4 15 2 20,55	4 17 18,78	28,8	40,5	41,0
14	W	10 16,56	12 30,4	13,84	12,06	28,92	34,5	35,5
16	E	8 21,44	6 1,6	19,84	18,07	28,8	39,0	39,9
ine 8	E	8 9,40	5 55,0	13,40	11,62	28,5	52,5	55,5
11	W	9 29,24	11 34,5	5,20	3 46	28,5	53,5	52,0
13	E	9 33,39	7 21,4	11,99	10 20	28,8	53,5	51,5
14	W	8 28,36	10 32,2	3,84	2,04	28,7	53,5	54,0
16	E	9 8,60	6 56,5	11,11	9,32	28,8	59,5	60,0
17	W	8 57,87	11 4,0	5,13	3,33	28,8	57,5	58,0
18	E	11 39,57	9 31,1	8,47	6 68	28,8	51,5	52,7 †
20	W	8 44,61	10 48,0	3,39	1,59	28 9	57,7	58,5
21	E	10 7,00	7 56,4	9,60	7,81	28,9	59,5	57,5

† Imperfect observation.

Observed Zenith Distance of 22 τ Herculis.

Point on the Limb, 3° 45′ South.

Day of the month	Face of the arch, W. or E.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above	Thermometer. Below
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
May 10	E	11 1,72	6 0,7	3 45 5 1,02	3 49 56,51	29,0	—	49,0
11	W	9 28,35	14 21,1	4 51,75	48,23	28,8	43,5	43,5
13	E	9 56,10	4 57,0	58,10	54,59	28,8	40,5	41,0
14	W	10 26,95	15 17,8	49,85	46,32	29,9	36,5	38 5
16	W	8 27,50	13 19,0	50,50	46,98	28,8	39,0	39,9
June 5	W	6 24,95	11 10,2	44,25	40,72	28,4	50,5	51,0
8	E	8 46,08	3 50,7	49,38	45,85	28,5	50,5	52,5
11	W	9 31,31	14 14,4	42,09	38,55	28 5	53,5	52,0
13	E	9 35,50	4 46,5	48,00	44,47	28,8	52,0	51,5
14	W	9 3,82	13 45,7	41,88	38,34	28,8	53,5	54 0
17	W	8 37,38	13 19,6	41,22	37,68	28,8	57,5	58,0
18	E	11 41,16	6 53,5	46,66	43,11	28,8	51,5	52,7
20	W	8 44,15	13 27,0	41,85	38,31	28,9	57,7	58,5
21	E	10 22,13	5 34,3	46,83	43,30	29,0	59,5	57,6

Observed Zenith Distance of Capella.

Point on the Limb, 4° 50′ South.

May 11	E	10 37,73	10 29,0	4 50 0 8,73	4 50 8,74	28,9	64,0	65,1
12	W	9 16,65	9 18,2	1,55	1,55	28,7	63,5	66,0
13	E	9 42,06	9 34,4	7,66	7,67	28,8	57,4	54,9
15	W	8 38,83	8 39,4	0,57	0,57	28,7	53,0	58,1
June 8	W	8 39,52	8 42,5	2,98	2,98	28,4	63,2	60,1
11	E	9 6,74	8 53,0	12,74	10,54	28,4	65,5	62,5
15	W	10 26,53	10 31,4	4,87	4,88	28,8	78,0	73,0
16	E	8 48,20	8 31,5	16,70	16,73	28,7	72,0	69,5
21	W	12 24,35	12 30,9	6,55	6,56	28,8	71,0	68,5
22	W	5 48,86	5 52,9	4,04	4,05	28,6	86,0	79,1

Operations at the Station near Clifton, the northern Extremity of the Arc, with the Zenith Sector. July and August, 1802.

On the 19th of July, the observatory and zenith sector were erected at the station, and the angle between the spindle of the weathercock on Laughton Spire and a staff at Gringley on

the Hill, was observed on different arches of the large theodolite; the results being as follows, *viz.* 78° 13′ 32″, 34, 35, 32 } Mean 78° 13′ 33″.

In a former article it has been shown, that Gringley is 76° 17′ 25″ south-east of the meridian of Clifton; therefore, 78° 13′ 33″ — 76° 17′ 25″ = 1° 56′ 8″, is the bearing of Laughton Spire from that meridian. The instrument, being otherwise adjusted for observation, was then brought into the plane of it, by setting off 1° 56′ 8″ on the azimuth circle; the permanency of the line of collimation of the lateral telescope having been previously ascertained.

Observations made at Clifton, to determine the Zenith Distance of β Draconis.

Point on the Limb, 1° 0′ South.

Day of the month.	Face of the arch, E. or W	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
July 20	W	12 1,04	12 14,8	1 0 0 13,76	1 0 13,78	28,8	58,0	56,0
22	W	7 53,33	13 12,0	17,67	15,52	28,7	54,0	54,5
26	E	13 27,55	13 6,8	20,75	20,78	28,8	64,2	64,3
28	W	9 21,94	9 32,3	10,36	10,38	28,8	59,5	58,5
29	E	9 3,13	8 44,1	18,03	18,06	28,8	56,5	57,5
31	W	9 34,59	9 44,1	9,51	9,52	29,0	57,2	56,5
Aug. 1	E	8 36,00	8 18,5	17,50	17,53	29,2	59,5	57,2
3	W	8 57,87	9 8,9	10,03	10,05	29,16	68,0	64,5
5	E	8 11,26	7 53,8	16,46	16,48	29,0	71,5	73,2
7	W	8 51,74	9 1,6	8,86	8,87	28,9	67,2	66,1
8	E	8 14,84	7 57,9	15,94	15,96	28,9	65,1	65,1
12	E	11 7,98	10 50,6	16,38	16,41	29,15	58,1	58,0
13	W	8 22,00	8 30,4	8,40	8,41	29,3	61,2	61,1
17	E	8 30,33	8 15,8	14,53	14,55	29,1	70,5	71,0
18	W	8 46,62	8 54,7	8,08	8,09	28,8	70,1	70,3

Observed Zenith Distance of γ Draconis.

Point on the Limb, 1° 55′ South.

Day of the month	Face of the arch, E. or W.	Plumb-line	Observation of the star.	Zenith distance in revolutions and parts	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
July 20	W	11 49,24	13 12,8	1 55 1 22,56	1 56 21,69	28,9	56,5	55,0
21	E	7 23,81	5 53,7	29,11	28,26	28,5	53,0	52,2
22	W	7 54,31	9 17,1	21,79	20,92	28,7	54,5	54,5
23	E	3 46,15	2 18,9	27,25	26,39	29,0	56,1	56,1
26	W	9 8,47	10 29,5	21,03	20,16	28,8	64,0	64,0
28	E	9 35,56	8 9,6	25,96	25,11	28,8	56,2	57,3
29	W	8 44,41	10 4,5	19,09	19,03	29,0	56,5	56,5
Aug. 1	W	8 41,22	10 3,0	20,78	19,91	29,2	59,5	57,0
3	E	9 7,59	7 40,3	26,29	25,43	29,1	68,0	64,5
5	E	7 50,50	6 25,0	25,50	24,64	29,0	73,0	71,0
7	W	9 7,55	10 24,6	17,05	16,18	28,9	64,2	65,2
12	E	11 7,56	9 42,7	23,86	23,00	29,1	57,5	57,5
13	W	8 12,48	9 29,4	16,92	16,04	29,3	63,0	61,2
17	E	8 10,32	6 46,0	23,32	22,46	29,0	69,5	70,5
18	W	8 32,97	9 48,5	15,53	14,65	28,8	70,0	70,1

Observed Zenith Distance of 45 d Draconis.

Point on the Limb, 3° 25′ North.

July 27	W	7 35,91	6 10,5	3 25 1 25,41	3 25 24,56	28,7	54,0	53,0
26	W	8 36,67	7 11,1	25,57	24,71	28,8	64,6	63,5
29	W	8 53,36	7 26,6	26,86	26,02	28,8	56,5	56,5
31	E	13 50,53	14 51,0	20,47	19,60	29,0	55,0	55,2
Aug. 7	W	8 47,50	7 18,6	28,90	28,04	28,9	65,0	64,0
12	E	11 9,60	12 31,0	21,40	20,53	29,2	55,5	55,5
13	W	8 10,99	6 38,9	31,09	30,24	29,3	60,1	59,1
17	E	8 14,53	9 38,9	24,37	23,51	29,0	71,0	69,5
18	W	8 15,03	6 41,3	32,73	31,88	28,8	—	68,0

Observed Zenith Distance of 46 c Draconis.
Point on the Limb, 1° 55′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer Above.	Thermometer Below
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
July 20	W	12 7,00	8 55,8	1 50 3 10,20	1 53 7,51	28,9	55,5	55,5
22	W	7 28,12	9 21,4	1 55 1 52,28	8,53	28,7	54,0	53,1
28	E	9 29,68	7 31,3	57,38	3,43	28,8	57,2	55,5
31	E	9 29,77	7 31,7	57,07	7,74	29,0	55,3	55,5
Aug. 3	E	8 55,40	7 0,6	54,80	6,01	29,1	64,0	63,5
5	E	7 11,47	5 15,8	54,67	5,14	29,0	73,5	71,5
7	W	6 37,64	10 26,6	47,96	12,86	28,9	65,2	64,2
12	E	11 3,15	9 9,0	53,15	7,66	29,2	55,5	59,1
13	W	8 6,41	9 52,5	46,09	14,73	29,3	60,0	59,0
17	E	8 25,75	6 32,3	52,45	8,36	29,0	71,2	69,5
18	W	8 20,25	10 6,3	45,05	15,78	28,8	71,0	68,0

Observed Zenith Distance of 51 Draconis.
Point on the Limb, 0° 20′ South.

July 28	E	9 31,75	7 50,7	0 20 1 40,05	0 29 39,21	28,8	55,0	55,0
31	E	9 29,90	7 48,5	40,04	39,56	29,0	55,0	55,5
Aug. 5	E	7 11,71	5 32,6	38,11	37,27	29,0	71,0	69,2
7	W	8 48,70	10 19,5	29,80	28,95	28,9	63,5	63,5
9	W	9 6,30	10 35,3	29,00	28,15	28,9	66,5	65,5
12	E	11 7,00	9 30,5	35,50	34,66	29,2	55,5	59,0
13	W	8 1,71	9 29,5	27,79	26,93	29,3	60,2	59,1
17	E	8 27,62	6 52,0	34,62	33,77	29,0	70,1	70,2
18	W	8 20,96	9 47,4	26,44	25,58	28,8	67,0	67,0

Observed Zenith Distance of μ Draconis.
Point on the Limb, 1° 15′ North.

July 20	W	11 51,87	10 9,3	1 15 1 42,57	1 16 41,73	28,9	58,0	56,0
28	W	9 11,90	7 26,9	44,00	43,17	28,8	58,2	58,5
29	E	9 11,32	10 51,0	39,68	38,84	28,8	56,5	57,5
30	W	9 55,53	8 10,0	45,53	44,70	28,8	59,0	57,5
Aug. 12	E	11 17,11	12 57,3	40,19	39,25	29,15	69,2	65,2
12	W	8 21,48	6 31,5	48,98	48,16	29,32	62,0	63,5

Observed Zenith Distances of 16 *Draconis.*

Point on the Limb, 0° 5′ *South.*

Day of the month.	Face of the arch, E. or W	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer Above.	Thermometer Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
July 29	E	9 26,41	6 31,7	2 53,71	2 53,99	28,8	59,7	59,
30	W	9 38,07	12 26,0	46,93	45,20	28,8	62,5	60,
Aug. 5	E	8 17,70	5 23,0	53,70	51,98	29,1	79,1	78,

Observed Zenith Distances of 1 *κ Cygni.*

Point on the Limb, 2° 25′ *North.*

July 20	W	13 45,26	15 42,5	0 25 1 56,24	0 26 55,43	28,9	56,5	55,
22	W	9 12,66	11 10,3	56,64	55,83	28,7	54,0	54,
26	W	9 42,42	11 39,1	55,68	54,87	28,8	64,5	63,
28	E	9 29,47	7 27,2	2 2,27	27 0,47	28,8	55,2	55,
29	W	9 3,65	11 7,4	3,75	1,95	28,9	56,9	55,
30	W	9 43,53	11 37,3	1 52,77	26 51,95	28,7	57,0	55,
31	E	9 30,77	7 28,5	2 2,27	27 0,47	29,0	55,0	55,
Aug. 5	E	7 8,34	5 9,1	1 58,24	26 57,43	29,0	71,2	69,
7	W	8 46,45	10 39,0	51,55	50,73	28,9	63,3	63,
9	W	8 32,50	10 24,6	51,10	50,28	28,9	66,0	65,
12	E	10 42,00	8 43,0	58,00	57,19	29,2	55,5	59,
13	W	8 1,11	9 50,9	49,79	48,97	29,3	60,0	59,
17	E	8 25,55	6 30,2	54,35	53,54	29,0	71,0	69,
18	W	8 27,85	10 17,0	48,15	47,33	28,8	66,0	66,

Observed Zenith Distances of 10 *ι Cygni.*

Point on the Limb, 2° 10′ *South.*

July 20	W	13 51,27	12 27,2	2 10 1 24,07	2 8 36,79	28,9	56,5	55,
28	E	9 22,60	10 42,5	19,90	40,97	28,8	57,3	55,
29	W	9 8,23	7 38,5	28,73	32,12	28,8	56,7	56,
30	W	9 31,50	8 4,0	27,50	33,36	28,7	55,5	57,
31	E	9 19,20	10 39,5	20,30	40,57	29,0	55,0	55,
Aug. 1	W	9 2,40	7 34,1	27,30	33,56	29,2	59,2	57,
5	E	7 0,70	8 23,2	22,50	38,36	29,0	68,5	68,
7	W	8 50,33	7 21,5	28,83	32,02	28,9	62,2	63,
9	W	8 47,48	7 18,0	29,48	31,37	28,9	65,5	66,
12	E	10 37,36	12 1,0	22,64	38,22	29,2	55,0	59,
17	E	8 21,50	9 47,0	25,50	35,36	29,0	61,9	68,
18	W	8 29,25	6 54,7	33,55	27,30	28,8	66,0	66,

Observed Zenith Distance of γ Ursæ.

Point on the Limb, 1° 20′ North.

Day of the month.	Face of the arch. E or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Aug. 17	E	8 58,45	9 9,9	1 20 0 10,45	1 20 10,42	29,3	89,0	83,6

Observed Zenith Distances of η Ursæ.

Point on the Limb, 3° 5′ South.

July 23	W	9 1,35	13 5,0	3 5 4 3,65	3 9 0,05	29,0	62,5	62,5
26	W	9 26,00	13 30,9	4,90	1,30	28,9	78,0	78,2
Aug. 4	W	8 40,50	12 46,0	5,50	1,90	29,2	79,3	79,5
8	E	7 34,74	3 22,0	17,74	9,15	29,0	76,0	73,0
17	E	8 52,05	4 38,5	13,45	9,86	29,14	88,0	81,5

Observed Zenith Distances of ζ Ursæ.

Point on the Limb, 2° 30′ North.

July 29	E	9 8,82	9 21,5	2 30 0 12,68	2 30 12,70	28,8	69,0	65,5
Aug. 5	W	8 40,97	8 22,5	18,47	18,50	29,1	79,5	78,0
8	E	8 25,15	8 37,0	11,83	11,87	29,0	69,0	65,5
9	W	8 49,00	8 29,4	14,60	19,62	28,9	80,0	80,0
17	E	9 10,43	9 22,0	11,57	11,59	29,4	85,1	80,1

Observed Zenith Distances of 85 ι Herculis.

Point on the Limb, 7° 20′ South.

July 21	W	11 50,28	12 6,0	7 20 0 14,72	7 20 14,74	28,9	55,5	55,5
23	E	3 50,07	3 28,5	11,57	21,60	29,0	56,1	56,1
2[illegible]	W	9 30,92	14 47,4	15 5 16,48	12,00	28,8	56,0	57,0
30	W	10 15,40	10 26,5	20 0 11,10	11,12	29,0	57,1	56,2
Aug. 1	W	9 6,18	9 17,0	10,82	10,84	29,2	59,5	57,2
5	E	8 2,52	7 42,0	19,52	19,75	29,0	73,0	71,0
7	W	9 17,54	9 29,0	11,46	11,48	28,9	67,2	66,1
17	E	8 15,52	7 56,0	18,52	18,55	29,0	70,5	71,2

Observed Zenith Distances of υ Herculis.
Point on the Limb, 6° 50′ South.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev div	° ′ rev. div.	° ′ ″	Inches.	°	°
July 21	E	8 11,5	6 16,6	6 50 1 53,7	6 51 52,89	28,5	55,5	54,5
29	E	9 14,1	7 17,0	56,1	55,29	28,8	60,7	61,5
30	W	7 57,8	9 48,8	46,0	47,17	28,8	62,5	60,0

Observed Zenith Distances of 52 Herculis.
Point on the Limb, 7° 5′ South.

July 28	W	8 58,72	11 15,5	7 5 2 15,78	7 7 14,00	28,8	58,5	58,5
29	E	9 11,35	6 47,0	23,35	21,58	28,8	59,7	59,5
30	W	10 1,29	12 16,0	14,71	12,93	28,8	61,0	59,0
Aug. 8	E	8 50,70	6 23,9	26,80	25,04	28,9	65,2	65,5

Observed Zenith Distances of 22 τ Herculis.
Point on the Limb, 6° 40′ South.

July 29	E	3 56,30	3 57,9	6 40 0 1,60	6 39 59,40	28,8	60,7	61,5
30	W	8 8,81	7 58,6	9,21	50,77	28,8	62,5	60,0
Aug. 1	W	8 5,20	7 53,1	11,10	48,88	29,2	67,0	67,0
7	W	10 1,73	9 50,5	10,23	49,75	28,9	71,0	69,0
12	W	10 16,30	10 4,0	12,50	47,48	29,2	65,2	63,3
13	E	8 12,30	8 16,8	4,50	55,49	29,3	67,3	66,5

Observed Zenith Distances of α Persei.
Point on the Limb, 4° 20′ South.

Aug. 8	W	9 7,66	7 45,7	4 20 1 20,96	4 18 39,91	28,9	66,5	63,0
10	W	8 38,84	7 18,0	20,84	40,03	28,9	70,2	71,0
13	E	10 27,76	11 45,6	17,84	43,03	29,3	57,0	54,0
18	E	8 26,58	9 43,5	16,92	43,95	29,0	60,2	60,2
19	W	8 11,42	6 47,5	22,92	37,94	28,8	60,5	60,3

Observed Zenith Distances of Capella.

Point on the Limb, 7° 40′ South.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below
		rev. div.	rev. div	° ′ rev. div.	° ′ ″	Inches.	°	°
Aug. 7	W	9 7,20	9 36,0	7 40 0 28,80	7 40 22,85	28,7	66,0	66,0
8	E	9 5,23	8 25,7	38,53	38,59	28,9	71,5	71,0
9	E	6 28,62	5 48,0	39,62	39 68	28,9	81,5	74,5
18	W	9 1,45	9 26,9	25,45	25,49	29,0	74,0	68,0
19	E	8 3,80	7 28,0	34,80	34,86	28,8	68,7	67,5

Operations at the new Station on Arbury Hill, near Daventry, with the Zenith Sector, in the Months of September and October, 1802.

In the Phil. Trans. for 1800, page 658, it will be seen, that the bearing of the Summer House on Bardon Hill, in the north of Leicestershire, from the meridian of Arbury Hill, is 7° 37′ 31″ NW; and, as this spot is only 2776 feet westward from the meridian of Dunnose itself, it follows, that 7° 37′ 31″ may be taken for the bearing of the above object from Arbury Hill. To avoid, however, the possibility of any error arising from adopting this supposition, the direction of the meridian was ascertained, (before the zenith sector was got up,) by a double azimuth of the pole star. From this it appeared, that the angular point of the roof of a house about seven miles distant, was within a few seconds of the true northern direction; and also, that Bardon Hill (the summer house) was 7° 37′ 35″ north-west. By observing these two objects, as the weather suited, the sector was afterwards got into the plane of the meridian.

Observations made on Arbury Hill, to determine the Zenith Distance of β Draconis.

Point on the Limb, 0° 15′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept. 8	W	9 48,90	10 54,7	0 15 1 5,80	0 13 55,09	28,2	51,0	54,0
18	E	9 37,98	8 26,5	11,48	49,40	28,8	70,5	72,5
19	W	9 17,78	10 23,0	5,22	55,67	28,8	71,5	76,5
20	E	9 16,33	8 6,5	9,83	51,06	28,8	68,2	69,0
22	E	9 26,17	8 16,0	10,17	50,71	28,8	79,3	75,3
23	W	8 21,00	9 25,0	4,00	56,89	28,9	76,5	76,5
24	E	9 7,68	7 57,0	9,68	51,21	28,9	71,0	70,5
25	W	9 29,13	10 34,0	4,87	56,02	29,1	74,5	75,5
26	E	9 4,27	7 51,8	11,47	49,41	29,0	64,5	66,5
28	W	10 43,25	11 48,7	5,45	55,45	29,0	65,5	65,5
29	E	9 27,65	8 17,3	10,35	50,53	29,1	79,0	77,5
30	W	9 25,82	10 30,7	4,88	56,04	29,0	64,0	69,5
Oct. 1	E	9 43,20	8 31,0	12,20	48,79	29,0	72,2	71,5
3	W	9 19,02	10 26,6	7,58	53,31	28,7	74,0	73,0

Observed Zenith Distances of γ Draconis.

Point on the Limb, 0° 40′ South.

Sept. 10	W	8 53,85	11 6,4	0 40 2 11,55	0 42 9,76	28,2	51,5	54,0
11	E	8 47,75	6 31,9	15,85	14,07	28,53	48,2	55,0
18	E	9 46,65	7 28,7	17,95	16,17	28,8	70,3	72,3
19	W	9 18,90	11 31,5	12,60	10,82	28,8	67,5	73,5
20	E	9 1,78	6 42,8	17,98	16,20	28,8	68,3	71,4
22	E	9 16,52	6 58,2	17,32	15,54	28,8	79,8	75,8
23	W	8 9,97	10 20,5	10,53	8,74	28,8	67,5	65,3
24	E	9 16,97	7 0,8	16,17	14,39	28,9	70,5	70,2
25	W	9 16,00	11 27,6	11,60	9,81	29,1	74,0	75,2
26	W	9 10,47	11 23,0	12,53	10,75	29,0	59,5	64,2
29	E	9 17,50	7 0,8	16,70	14,92	29,1	64,0	69,5
30	W	9 21,63	11 33,5	11,87	10,08	29,9	64,0	69,5
Oct. 1	E	9 34,95	7 15,5	19,45	17,87	28,9	72,5	71,9
2	E	9 25,33	7 7,0	18,33	16,57	28,8	71,0	75,0
3	W	8 54,30	11 7,1	11,80	10,01	28,6	74,0	73,0

Observed Zenith Distances of 45 d Draconis.

Point on the Limb, 4° 40′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept. 8	W	9 57,57	9 21,1	4 40 0 36,47	4 40 38,53	28,2	52,0	54,0
15	W	7 29,48	6 47,3	40,98	41,05	28,9	63,5	66,0
18	E	9 45,55	10 18,6	32,05	32,10	28,8	64,2	67,2
19	W	9 28,10	8 46,6	40,50	40,57	28,8	66,5	72,5
20	E	8 56,23	9 29,1	31,87	31,92	28,8	66,5	69,5
23	W	8 0,83	7 22,4	37,43	37,49	28,8	67,5	65,5
24	E	9 18,78	9 50,4	31,62	31,67	28,9	65,5	63,5
25	W	9 36,25	8 57,1	38,15	38,21	29,0	65,3	67,3
26	E	9 14,12	9 46,5	32,37	32,42	29,0	64,8	66,5
28	E	9 18,62	9 50,0	31,38	31,43	29,0	65,3	64,5
29	W	8 57,55	8 20,0	37,55	37,61	29,1	64,5	69,0
30	W	8 51,62	8 13,5	37,12	38,18	29,0	64,3	69,8
Oct. 1	E	9 25,05	14 58,7	33,65	29,20	28,9	72,0	71,5
2	E	9 18,35	9 50,6	32,35	32,40	28,8	72,5	75,5

Observed Zenith Distances of 46 c Draconis.

Point on the Limb, 3° 5′ North.

Sept. 7	E	9 3,63	11 19,23	3 5 2 15,40	3 7 13,62	28,5	63,0	64,
10	E	9 3,73	11 19,4	15,67	13,89	28,2	51,0	54,
15	W	7 30,08	5 6,5	23,58	21,85	28,9	63,5	66,
16	W	10 22,70	7 55,9	24,80	23,04	29,0	61,5	65,
18	E	9 29,12	11 47,0	17,88	16,11	28,8	64,0	67,
19	W	9 40,57	7 16,3	24,27	22,51	28,8	66,5	72,
20	E	8 34,27	10 52,3	18,03	16,26	28,8	66,0	69,
21	W	9 55,90	7 29,0	26,90	25,14	28,8	66,5	69,
22	E	8 2,[illegible]	[illegible] 20,4	17,73	15,95	28,8	79,2	75,
23	W	9 20,01	6 45,0	24,10	22,34	28,8	67,5	65,
[illegible]	[illegible]	[illegible] 3,97	11 22,3	18,33	16,56	28,8	65,5	63,
[illegible]	[illegible]	[illegible]	7 19,2	23,35	21,58	29,0	65,5	67,
[illegible]	[illegible]	[illegible]	[illegible] 27,0	17,90	16,12	29,0	64,5	66,
[illegible]	[illegible]	[illegible]	[illegible]	17,48	15,70	29,0	65,5	64,
29	[illegible]	[illegible] 23,00	7 48,4	23,00	21,83	29,0	64,5	69
30	[illegible]	[illegible]	[illegible]	24,40	22,64	29,1	64,5	69,
1	[illegible]	[illegible]	[illegible] 36,1	17,45	15,67	[illegible]	68,[illegible]	61
2	[illegible]	[illegible]	[illegible]	18,75	16,97	[illegible]	68,0	70

Observed Zenith Distances of 1 κ *Cygni.*

Point on the Limb, 0° 45′ *North.*

Day of the month.		Face of the arch, E or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer. Above.	Thermometer. Below.
			rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept.	7	E	8 12,72	10 25,2	0 45 2 12,48	0 47 10,69	28,2	51,0	54,0
	8	W	6 54,92	4 36,0	18,92	17,15	28,2	51,5	54,5
	15	W	8 52,42	6 31,5	20,92	19,15	28,9	62,5	65,3
	16	W	10 31,57	8 9,0	22,57	20,80	28,9	61,0	65,0
	18	E	8 21,80	10 38,0	16,20	14,42	28,8	64,3	67,5
	19	W	9 37,03	7 14,5	22,53	20,77	28,8	66,5	72,0
	20	E	8 24,05	15 43,6	40 7 19,55	13,27	28,8	65,0	67,0
	22	E	8 50,82	11 9,0	45 2 17,18	15,40	28,8	66,3	66,5
	23	W	8 56,10	6 33,5	22,60	20,83	28,8	67,5	65,3
	24	E	8 32,52	10 48,5	15,98	14,20	28,9	59,3	63,5
	25	W	9 44,57	7 23,5	21,07	19,30	29,0	66,5	67,0
	26	E	9 20,58	11 36,0	15,42	13,64	29,0	64,5	66,5
	28	E	9 31,90	11 47,7	15,80	14,02	29,0	60,5	62,5
	29	W	9 58,00	7 35,0	23,00	21,23	29,0	64,5	68,0
	30	W	9 48,10	7 24,9	23,20	21,43	29,0	62,0	65,5
Oct.	1	E	9 9,42	11 26,0	16,58	14,80	28,9	64,0	66,5
	2	E	9 21,82	11 38,0	16,18	14,40	28,0	65,0	68,0

Observed Zenith Distances of 51 *Draconis.*

Point on the Limb, 0° 50′ *North.*

Sept.	7	E	8 13,88	5 43,5	0 55 2 29,38	0 52 32,37	28,2	51,5	54,5
	8	W	6 41,63	4 1,5	0 50 2 49,13	38,39	28,2	51,0	54,0
	10	E	9 16,00	11 53,7	37,70	35,96	28,2	51,0	54,0
	16	W	10 39,90	7 55,5	42,50	40,77	29,0	61,7	65,5
	18	E	8 42,00	11 19,7	36,70	34,96	28,8	64,0	67,2
	19	W	9 56,73	7 19,6	43,13	41,42	28,8	66,5	72,0
	20	E	8 26,10	11 13,0	35,90	34,15	28,8	66,5	69,5
	22	E	8 54,28	11 31,2	35,92	34,17	28,8	68,5	63,0
	23	W	9 4,53	6 21,9	41,63	39,89	28,8	67,5	65,5
	25	W	9 34,53	6 31,5	42,03	40,30	29,0	67,0	[illegible]
	26	E	9 24,17	12 [illegible]	36,23	[illegible]	29,0	[illegible]	[illegible]
	28	E	9 30,17	12 [illegible]	36,13	[illegible]	[illegible]	[illegible]	64,0
	[illegible]	W	9 17,85	6 53,9	40,95	39,21	29,0	[illegible]	69,5
	[illegible]	W	9 [illegible]	6 51,0	[illegible]	[illegible]	[illegible]	[illegible]	70,3
Oct.	[illegible]	E	9 [illegible]	11 49,0	[illegible]	35,71	[illegible]	68,0	68,0
	[illegible]	E	9 [illegible]	11 2,4	[illegible]	35,04	[illegible]	68,0	70,5

Observed Zenith Distance of 10 ι Cygni.

Point on the Limb, 0° 55′ South.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept. 18	E	8 19,17	8 53,6	0 55 0 34,43	0 54 25,51	28,8	64,5	67,5
19	W	9 37,65	9 2,2	35,45	20,49	28,8	66,5	72,0
20	E	8 31,87	9 7,5	34,63	25,31	28,8	65,0	67,0
21	W	10 16,65	9 35,3	40,35	19,58	28,8	65,2	67,2
22	E	8 51,59	9 28,2	35,61	24,33	28,8	66,0	66,0
23	W	8 49,25	8 10,5	38,75	21,19	28,8	65,5	65,5
24	E	8 22,08	8 56,0	33,91	26,02	28,8	59,0	63,0
25	W	9 45,92	9 7,0	38,92	21,02	29,0	66,5	67,0
26	E	9 11.63	9 46,4	34,77	25,17	29,0	59,0	64,0
27	W	8 57,40	8 13,4	44,00	19,93	29,0	52,0	55,5
28	E	8 54,83	9 31,1	35,27	24,67	29,0	60,0	62,0
29	W	9 52,90	9 [illegible]	40,90	19,03	29,0	64,5	68,0
30	W	9 53,38	9 [illegible]	42,08	17,85	29,0	62,0	65,0
Oct. 1	E	9 10,95	9 47,0	36,05	23,89	28,9	64,0	66,5
2	E	9 23,18	10 0,3	36,12	23,82	28,8	68,5	65,5

Observed Zenith Distances of γ Ursæ.

Point on the Limb, 2° 35′ North.

Sept. 18	E	9 32,47	8 34,9	2 35 0 56,57	2 34 3,33	28,8	77,5	76,5
[illegible]	W	8 20,07	9 20,1	2 35 0 57,03	2,87	28,9	80,0	76,0
[illegible]	W	9 22,70	10 18,5	54,80	5,10	29,1	80,5	75,0
Oct. 3	E	9 10,45	8 2,5	1 7,95	33 52,94	28,7	72,8	78,5

Observed Zenith Distances of ζ Ursæ.

Point on the Limb, 3° 45′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer	Thermometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept. 24	E	8 40,70	7 44,4	3 45 0 55,60	3 44 4,31	28,9	79,5	75,5
26	W	9 1,95	9 53,5	51,55	8,36	29,1	80,0	77,5
30	E	9 50,72	8 50,0	1 0,72	0,18	29,0	84,0	76,0
Oct. 3	E	7 3,00	6 1,9	1 1,10	43 59,80	28,7	80,5	75,5

Observed Zenith Distances of 22 τ Herculis.

Point on the Limb, 5° 15′ South.

Sept. 18	E	9 50,42	8 52,5	5 25 0 56,92	5 25 57,01	28,8	72,5	74,5
Oct. 3	W	9 33,10	10 24,5	50,40	50,48	28,6	76,5	76,0

Observed Zenith Distances of α Persei.

Point on the Limb, 3° 5′ South.

Sept. 8	E	7 35,23	8 0,6	3 5 0 23,37	3 4 36,59	28,8	41,0	44,0
12	E	8 29,27	8 52,5	23,23	36,73	28,8	41,5	44,5
16	W	9 17,50	8 47,9	28,00	31,35	28,9	55,7	57,5
18	W	9 53,52	9 25,3	28,22	[illegible]	28,8	57,2	[illegible]
[illegible]	[illegible]	7 52,87	8 11,0	23,13	[illegible]	[illegible]	[illegible]	58,0
[illegible]	[illegible]	7 44,73	8 [illegible]	23,57	[illegible]	[illegible]	[illegible]	62,0
[illegible]	W	8 [illegible]	[illegible]	[illegible]	[illegible]	28,7	60,0	60,0
[illegible]	E	8 [illegible]	[illegible]	[illegible]	[illegible]	29,0	49,5	53,5
26	W	9 [illegible]	[illegible]	[illegible]	30,65	29,1	55,5	54,3

Observed Zenith Distances of Capella.

Point on the Limb, 7° 40′ South.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer.	Thermometer Above.	Thermometer Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°	°
Sept. 11	E	9 47,77	8 15,9	6 25 1 33,87	6 26 33,02	28,8	46,5	50,5
12	E	8 45,17	7 12,1	33,07	32,22	28,7	38,5	43,5
14	W	7 41,57	9 9,1	26,53	25,67	28,8	53,5	56,5
16	W	9 51,50	10 20,3	27,80	26,94	28,9	54,5	56,5
18	E	9 24,77	7 48,8	34,97	34,12	28,9	55,0	58,0
19	W	8 10,88	9 38,0	27,52	26,66	28,9	56,0	57,5
20	E	8 48,20	7 15,2	33,00	32,15	28,8	57,2	59,1
21	W	8 16,97	9 45,5	28,53	27,67	28,8	54,0	56,5
22	E	9 6,93	7 34,0	31,93	31,08	28,8	58,0	62,0
23	W	8 48,50	10 16,2	26,70	25,84	28,7	60,5	58,5
25	E	8 53,40	7 20,0	33,30	32,45	29,0	48,5	48,5
26	W	9 40,52	11 18,6	28,08	27,22	29,1	55,0	56,5

Operations at the Royal Observatory with the Zenith Sector April, 1802.

Observed Zenith Distances of β Draconis.

Point on the Limb, 0° 55′ North.

April 16	W	9 57,80	6 44,2	0 55 3 13,60	0 58 10,92	29,9	40,0	40,0
23	E	8 35,49	11 40,9	5,41	2,73	30,1	38,0	38,0
25	W	10 7,84	6 53,0	13,84	11,16	29,8	44,0	44,0
26	W	[illegible]	6 11,5	13,13	10,45	29,5	42,0	42,0

Observed Zenith Distances of 45 Draconis.
Point on the Limb, 5° 20′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts.	Zenith distance reduced.	Barometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°
April 19	W	9 38,57	4 46,5	5 20 4 51,07	5 24 47,48	31,1	53
23	E	8 21,37	13 6,0	43,63	40,01	30,1	38
25	W	9 47,20	4 54,5	51,70	48,19	29,8	40

Observed Zenith Distance of 46 [illegible] Draconis.
Point on the Limb, 3° 50′ North.

April 15	W	9 15,70	7 41,9	3 50 1 32,80	3 51 31,95	29,8	44

Observed Zenith Distance of 51 Draconis.
Point on the Limb, 1° 36′ North.

April 19	W	9 48,80	8 55,7	1 35 0 52,10	1 35 52,19	31,1	53
23	E	8 45,56	16 27,8	43,34	43,31	30,1	38
25	W	8 54,53	7 46,0	49,83	49,91	29,8	44

Observed Zenith Distance of 1 κ Cygni.
Point on the Limb, 1° 30′ North.

April 23	E	9 20,36	10 42,8	1 30 1 22,44	1 31 21,53	30,1	38
25	W	8 29,76	6 58,6	30,16	29,31	29,5	51

Observed Zenith Distance of 10 [illegible] Cygni.
Point on the Limb, 0° 10′ South.

April 19	W	11 32,42	11 42,6	0 19 0 10,18	0 10 10,19		
23	E	9 36,12		18,42	18,45		

Observed Zenith Distance of γ Ursæ.

Point on the Limb, 3° 15′ North.

Day of the month.	Face of the arch, E. or W.	Plumb-line.	Observation of the star.	Zenith distance in revolutions and parts	Zenith distance rectified.	Barometer. Above.	Thermometer. Below.
		rev. div.	rev. div.	° ′ rev. div.	° ′ ″	Inches.	°
April 20	E	8 44,30	12 42,7	3 15 3 58,40	3 18 55,79	29,9	50
22	E	10 36,00	14 36,0	4 0,00	56,39	29,9	50
23	E	4 28,25	8 29,5	0,75	57,14	30,1	48
24	W	9 36,90	5 25,1	11,80	19 8,21	29,8	47

Observed Zenith Distance of η Ursæ.

Point on the Limb, 1° 10′ South.

April 16	W	10 13,90	10 40,7	1 10 0 26,80	1 10 26,84	29,9	45
23	E	3 37,50	3 4,6	32,40	32,45	30,1	38

Table showing the Runs of the Micrometer-screw over every 5′ in the first Degree on each Side of Zero.

Right Hand Arc.

	°	′	R.	D.	R.	D.
At	0	0	8	55,43	5	4,45
	0	5	14	0,88		
	0	5	9	32,55	5	4,55
	0	10	14	37,10		
	0	10	9	40,03	5	4,34
	0	15	14	44,37		
	0	15	9	19,13	5	4,45
	0	20	14	23,58		
	0	20	9	54,07	5	4,40
	0	25	14	58,47		
	0	25	9	39,23	5	4,41
	0	30	14	43,64		
	0	30	9	25,77	5	4,44
	0	35	14	30,21		
	0	35	9	58,53	5	4,54
	0	40	15	4,07		
	0	40	9	0,53	5	4,54
	0	45	14	5,07		
	0	45	9	12,47	5	4,55
	0	50	14	17,02		
	0	50	9	43,07	5	4,43
	0	55	14	47,50		
	0	55	8	41,27	5	4,50
	1	0	13	45,77		

Left Hand Arc.

	°	′	R.	D.	R.	D.
At	0	0	9	16,31	5	4,54
	0	5	4	11,77		
	0	5	9	8,73	5	4,56
	0	10	4	4,17		
	0	10	8	53,67	5	4,50
	0	15	3	49,17		
	0	15	9	16,13	5	4,44
	0	20	4	11,69		
	0	20	9	17,50	5	4,53
	0	25	4	12,97		
	0	25	10	4,30	5	4,50
	0	30	4	58,80		
	0	30	8	52,0	5	4,47
	0	35	3	47,53		
	0	35	9	7,83	5	4,53
	0	40	4	3,30		
	0	40	9	3,31	5	4,41
	0	45	3	57,90		
	0	45	9	12,63	5	4,40
	0	50	4	8,23		
	0	50	9	4,50	5	4,47
	0	55	4	0,03		
	0	55	8	35,0	5	4,57
	1	0	3	30,43		

Table for converting the Divisions shewn on the Micrometer Head into Seconds; the Space subtended by 5′ on the Limb being found = 5 Revolutions 45 Divisions, as deduced from the Measurement of the Total Arches.

R.	D.	″	R.	D.	″
0	1	1,002	0	30	30,050
0	2	2,003	0	31	31,052
0	3	3,005	0	32	32,053
0	4	4,007	0	33	33,055
0	5	5,008	0	34	34,057
0	6	6,010	0	35	35,058
0	7	7,012	0	36	36,060
0	8	8,013	0	37	37,062
0	9	9,015	0	38	38,063
0	10	10,016	0	39	39,065
0	11	11,018	0	40	40,067
0	12	12,020	0	41	41,068
0	13	13,022	0	42	42,170
0	14	14,023	0	43	43,072
0	15	15,025	0	44	44,073
0	16	16,027	0	45	45,075
0	17	17,028	0	46	46,077
0	18	18,030	0	47	47,078
[illegible]	19	[illegible]	[illegible]	[illegible]	48,080
[illegible]	[illegible]	[illegible]	[illegible]	49	49,082
			0	50	50,083
			0	51	51,085
			0	52	[illegible],087
			0	53	53,088
			0	54	[illegible]
			0	55	[illegible]
			[illegible]	[illegible]	[illegible]

Table for supplying the necessary Correction to the observed Zenith Distance of a Star, on account of the Expansion or Contraction of the sectorial Tube by 1° of Heat.

Zenith distance observed.	Correction for 1° of heat.	Zenith distance observed.	Correction for 1° of heat.
° ′	″	° ′	″
1 —	0,018	4 30 —	0,084
1 30 —	0,028	5 —	0,093
2 —	0,037	5 30 —	0,102
2 30 —	0,046	6 —	0,111
3 —	0,056	6 30 —	0,121
3 30 —	0,065	7 —	0,130
4 —	0,074	7 30 —	0,129

In using the above Table, the corrections are to be taken as negative, if the upper thermometer denotes the air to be hotter towards the top of the observatory than round the limb of the sector; and positive, if the reverse.

Reduction of the several Observations contained in the preceding Article, fr the respective Days on which they were made, to the first of January, 18c the Equations being those for Aberration, Nutation, semi-annual solar Equati Precession, and Refraction; with the Zenith Distances of the several St deduced therefrom.

Reduction of the Observations made at Dunnose.

β Draconis, N.

ace of limb, West.		Face of limb, East.				
lay 11—1° 50′	7″,65	May 13—1° 50′	0″,23	Zenith dist. 1° 50′ 3″,46		
14	7,0	16	1,10		+1,83	Mean refraction.
une 5	6,11	June 8	0,12		−0,05	Temperature.
					+0,00	Expansion of axis.
11	6,99	13	0,55	Mean zen. dist. 1 50 5,24.		
14	5,87	16	0,01″	Line of collimation 3,42.		
17	6,32	18	49 58,59			
20	7,43	21	0,68			
Mean 1 50	6,88	Mean 1 50	0,04			

γ Draconis, N.

lay 11—0° 54′	0″,34	May 10—0° 53′	51″,66	Zenith dist. 0° 53′ 55″,75		
14	0,12	13	52,31		+ 0,91	Refraction.
une 11	53 59,45	16	51,54		− 0,02	Temperature.
					− 0,01	Expansion of axis.
14	59,14	June 13	51,53	Mean zen. dist. 0 53 56,63.		
17	59,44	17	53,58	Line of collimation 3,64.		
20	57,83	18	51,11			
		21	53,07			
Mean 0 53	59,39	Mean 0 53	52,11			

51 *Draconis, N.*

Face of limb, West	Face of limb, East.
June 14—2° 28′ 45″,62	June 13—2° 28′ 38″,26
18 44,85	10 37,67
21 44,85	20 39,01
Mean 2 28 45,11	Mean 2 28 38,31

Zenith dist. 2° 28′ 41″,71 + 2″,34 (refr. &c.) = 2° 28′ 44″,05.
Line of collimation 3,40.

μ *Draconis, N.*

May 11—4° 7′ 1″,57	May 13—4° 6′ 51″,29
14 6 57,63	June 8 51,58
June 14 59,13	13 51,00
17 58,18	16 51,12
20 58,43	18 52,20
	21 52,57
Mean 4 6 58,99	Mean 4 6 51,62

Zenith dist. 4° 6′ 55″,30 + 4″, (refr. &c.) = 4° 6′ 59″,30
Line of collimation 3,68,

16 *Draconis, N.*

May 11—2° 42′ 34″,99	June 13—2° 42′ 26″,55
14 34,17	16 27,73
16 33,27	18 26,85
June 11 34,25	
14 34,26	
20 34,31	
Mean 2 42 34,21	Mean 2 42 27,04

Zenith dist. 2° 42′ 30″,63 + 2″,63 (refr. &c.) = 2° 42′ 33″,2
Line of collimation 3,58.

1 κ *Cygni, N.*

June 14—2° 23′ 26″,44	June 13—2° 23′ 16″,46
18 23,20	16 16,54
21 20,51	20 17,31
Mean 2 23 24,38	Mean 2 23 16,77

Zenith dist. 2° 23′ 20″,58 + 2″,28 (refr. &c.) = 2° 23′ 22″,
Line of collimation 3,80.

10 ι *Cygni, N.*

June 14—0° 41′ 43″,52	June 13—0° 41′ 35″,84
18 43,53	16 36,42
21 42,69	20 38,54
Mean 0 41 43,24	Mean 0 41 36,93

Zenith dist. 0° 41′ 40″,08 + 0″,68 (refr. &c.) = 0° 41′ 40″,
Line of collimation 3,15.

γ *Ursæ, N.*

May 11—4° 10′ 33″,47	May 9—4° 10′ 28″,18
15 36,73	10 28,58
June 5 36,65	13 29,82
13 35,75	14 [illegible],01
14 3[illegible],80	17 30,01
	20 29,44
	June 8 30,24
	[illegible] 30,24
Mean 4 10 35,48	[illegible] 4 10 29,44

Zenith dist. 4° 10′ 32″,46 + 3″,77 (refr. &c.) = 4° 10′ 36″
Line of collimation 3,02.

η Ursæ, S.

Face of limb, West.		Face of limb, East.		
May 14	—0° 18′ 38″,84	May 10	—0° 18′ 44″,08	Zenith dist. 0° 18′ 42″,61 + 0″,32 (refr. &c.) = 0° 18′ 42″,9
16	40,16	13	45,86	Line of collimation 3,06.
June 5	39,64	15	46,11	
11	37,13	17	45,01	
13	38,62	June 8	47,04	
16	42,06	12	46,57	
18	39,89	14	45,21	
		20	46,18	
		21	43,78	
Mean	0 18 39,48	Mean	0 18 45,54	

ζ Ursæ, N.

May 11	—5° 20′ 34″,10	May 13	—5° 20′ 26″,41	Zenith dist. 5° 20′ 30″,53 + 5″,13 (refr. &c.) = 5° 20′ 35″,6
June 5	34,15	17	28,46	Line of collimation 3,76.
11	34,42	June 8	25,28	
18	34,57	14	26,96	
		17	26,38	
		20	27,14	
Mean	5 20 34,30	Mean	5 20 26,77	

85 *ι Herculis, S.*

May 14	—4° 29′ 54″,76	May 10	—4° 30′ 0″,16	Zenith dist. 4° 29′ 57″,48 + 4″,47 (refr. &c.) = 4° 30′ 1″,9
June 14	53,20	13	1,39	Line of collimation 3,46.
		16	1,18	
Mean	4 29 53,98	Mean	4 30 0,91	

υ Herculis, S.

May 14	—4° 1′ 25″,39	May 13	—4° 1′ 35″,50	Zenith dist. 4° 1′ 29″,55 + 3″,69 (refr. &c.) = 4° 1′ 33″,2
16	26,46	June 13	32,52	Line of collimation 3,35.
June 5	26,51	16	32,56	
14	26,51	18	32,56	

22 τ *Herculis.*

Face of limb, West.		Face of limb, East	
May 11	—3° 49′ 30″,31	May 10	—3° 49′ 38″,10
14	29,26	13	37,23
16	30,63	June 8	35,04
June 5	29,92	13	35,88
11	29,43	18	35,86
14	30,09	21	36,83
17	29,86		
20	31,58		
Mean	3 49 30,16	Mean	3 49 36,49

Zenith dist. 3° 49′ 33″,32 + 3″,78 (refr. &c.) = 3° 49′ 37″,10.
Line of collimation 3,16.

Capella, S.

May 12	—4° 50′ 55″,46	May 11	—4° 50′ 2″,79
15	54,02	13	1,47
June 8	54,87	June 11	2,0
15	55,87	16	2,26
21	56,91		
22	55,24		
Mean	4 50 55,49	Mean	4° 50 2,13

Zenith dist. 4° 59′ 58″,81 + 4″,07 (refr. &c.) = 4° 50′ 2″,88.
Line of collimation 3,31.

Reduction of the Observations made at Clifton, (the northern Extremity of the meridional Arc,) and the Zenith Distances of the several Stars deduced therefrom.

β *Draconis, S.*

July 20	—1° 0′ 13″,82	—July 26	—1° 0′ 22″,41
22	13,15	29	20,26
28	12,38	Aug. 1	20,39
31	12,15	5	20,16
Aug. 3	13,29	8	20,15
7	12,87	12	21,25
13	13,42	17	20,11
18	13,80		
Mean	1 0 13,11	Mean	1 0 20,68

Zenith dist. 1° 0′ 16″,89 + 0″,95 (refr. &c.) = 1° 0′ 17″,84.
Line of collimation 3,78.

χ *Draconis, S.*

July 20	—1° 56′ 21″,63		1° 56′ 28″,50
22	21,47		27,14
26	21,72		[illegible]
29	21,67		28,86
Aug. 1	22,03		28,34
11	20,45		28,28
13	21,55		28,66
18	21,03		
Mean	1 56 21,56	Mean	1 56 28,16

Zenith dist. 1° 56′ 24″,86 + 1″,78 (refr. &c.) = 1° 56′ 26″,64.
Line of collimation 3,30.

45 *d Draconis, N.*

Face of limb, West.	Face of limb, East.	
July 22—3° 26′ 23″,96	July 31—3° 26′ 16″,41	Zenith dist. 3° 26′ 19″,63 + 3″,29 (refr. &c.) = 3° 26′ 22″,92.
26 22,92	12 15,23	Line of collimation 3,72.
29 23,35	17 16,09	
Aug. 7 23,0		
13 23,7		
18 23,26		
Mean 3 26 23,36	Mean 3 26 15,91	

46 *c Draconis, N.*

July 20—1° 53′ 7″,60	July 28—1° 53′ 1″,11	Zenith dist. 1° 53′ 4″,44 + 1″,80 (refr. &c.) = 1° 53′ 6″,24.
22 8,01	31 52 59,97	Line of collimation 3,47.
Aug. 7 7,77	Aug. 3 53 1,99	
13 8,20	5 0,59	
18 8,04	12 1,34	
	17 0,84	
Mean 1 53 7,92	Mean 1 53 0,97	

51 *Draconis, S.*

Aug. 7—0° 21′ 33″,26	July 28—0° 21′ 41″,62	Zenith dist. 0° 21′ 37″,78 + 0″,34 (refr. &c.) = 0° 21′ 38″,12.
9 33,99	31 42,87	Line of collimation 4,15.
13 33,83	Aug. 5 42,16	
18 33,45	12 41,30	
	17 41,68	
Mean 0 21 33,63	Mean 0 21 41,93	

μ Draconis, N.

July 20—1° 16′ 39″,87	July 29—1° 16′ 34″,53	Zenith dist. 1° 16′ 36″,97 + 1″,23 (refr. &c.) = 1° 16′ 38″,20.
28 38,98	32,92	Line of collimation 3,25.
30 40,52		
Aug. 13 41,73		
Mean 1 16 40,22	Mean 1 16 33,72	

16 *Draconis, S.*

July 30—0° 7′ 47″,75	July 29—0° 7′ 54″,09	Zenith dist. 0° 7′ 51″,15 + 0″,10 (refr. &c.) = 0° 7′ 51″,25
	Aug. 5 0 55,[illegible]	Line of collimation 3,40.
	Mean 0 7 [illegible]	

10 ι *Cygni, S.*

Face of limb, West.	Face of limb, East.	
July 20—2° 8′ 36″,86	July 28—2° 8′ 42″,47	Zenith dist. 2° 8′ 40″,23+1″,99 (refr. &c.)=2° 8′ 42″,22.
29 35,01	31 44,05	Line of collimation 3,55.
30 36,42	Aug. 5 43,54	
Aug. 1 37,37	12 45,24	
7 37,62	17 43,76	
9 37,56		
18 35,96		
Mean 2 8 36,68	Mean 2 8 43,79	

γ *Ursæ, N.*

Aug. 17—1° 20′ 8″,84.

ζ *Ursæ, N.*

Aug. 5—2° 30′ 10″,72	July 29—2° 30′ 4″,59	Zenith dist. 2° 30′ 8″,18+2″,19 (refr. &c.)=2° 30′ 10″,37
9 12,38	Aug. 8 4,47	Line of collimation 3,37.
	17 5,39	
Mean 2 30 11,55	Mean 2 30 4,81	

η *Ursæ, S.*

July 23—3° 8′ 59″,78	Aug. 8—3° 9′ 8″,03	Zenith dist. 3° 9′ 4″,26+2″,72 (refr. &c.)=3° 9′ 6″,98
26 9 0,93	17 7,85	Line of collimation 3,67.
Aug. 4 9 1,07		
Mean 3 9 0,59	Mean 3 9 7,94	

85 ι *Herculis, S.*

July 20—7° 20′ 14″,13	July 23—7° 20′ 21″,69	Zenith dist. 7° 20′ 18″,08+6″,90 (refr. &c.)=7° 20′ 24″,98
28 13,19	5 22,57	Line of collimation 4,52.
31 12,93	17 23,59	
Aug. 1 12,86		
7 14,71		
Mean 7 20 13,56	Mean 7 20 22,61	

υ *Herculis, S.*

July 30—6° 51′ 46″,31	July 21—6° 51′ 52″,89	Zenith dist. 6° 51′ 50″,45+6″,35 (refr. &c.)=6° 51′ 56″,80
	29 56,32	Line of collimation 4,14.
	Mean 6 51 54,60	

52 *Herculis, S.*

July 28—7° 7′ 15″,02	July 29—7° 7′ 22″,76	Zenith dist. 7° 7′ 18″,69+6″,76 (refr. &c.)=7° 7′ 25″,4
30 14,21	Aug. 8 22,63	Line of collimation [illegible]
Mean 7 7 14,6	Mean 7 7 22,69	

[illegible] τ *Herculis, S.*

July 30—6° 39′ 51″,84	July 29—6° 40′ [illegible]	Zenith dist. 6° 39′ 55″,[illegible]+6″,18 (refr. &c.)=6° 40′ 1″,2
Aug. 4 50,64	Aug. 13 39 58,0[illegible]	[illegible] collimation 4,16.
7 51,80		
12 49,96		
Mean 6 39 51,06	Mean 6 39 59,17	

α *Persei*, S.

Face of limb, West.		Face of limb, East.		
Aug. 8—4° 18′ 29″,44		Aug. 13—4° 18′ 33″,15		Zenith dist. 4° 18′ 31″,65 + 4″,37 (refr. &c.) = 4° 18″ 36′,02.
	29,78	18	34,76	Line of collimation 2,28.
	28,87			
Mean 4 18 29,36		Mean 4 18 33,95		

Capella, S.

Aug. 7—7° 40′ 15″,60		Aug. 8—7° 40′ 25″,30		Zenith dist. 7° 40′ 19″,06 + 6″,60 (refr. &c.) = 7° 40′ 25″,66.
18	11,94	9	26,46	Line of collimation 5,30.
		19	21,32	
Mean 7 40 13,76		Mean 7 40 24,36		

Reduction of the Observations made at Arbury Hill, (the intermediate Point on the meridional Arc,) and the Zenith Distances of the several Stars deduced therefrom.

β *Draconis*, N.

Sept. 8—0° 13′ 47″,67		Sept. 18—0° 13′ 41″,91		Zenith dist. 0° 13′ 45″,61 + 0″,21 (refr. &c.) = 0° 13′ 45″,82.
19	48,20	20	43,61	Line of collimation 2,71.
23	49,55	22	43,32	
25	48,76	24	43,90	
28	48,38	26	42,24	
30	49,10	29	43,54	
Oct. 3	46,63	Oct. 1	41,94	
Mean 0 13 48,33		Mean 0 13 42,92		

γ *Draconis*, S.

Sept. 10—0° 42′ 18″,72		Sept. 13—0° 42′ 23″,08		Zenith dist. 0° 42′ 22″,08 + 0″,65 (refr. &c.) = 0° 42′ 22″,73.
19	20,01	18	25,43	Line of collimation 2,92.
23	18,03	20	25,49	
25	19,07	22	24,84	
26	20,97	24	24,67	
30	10.21	20	24,[illegible]	

46 *c Draconis, N.*

Face of limb, West.		Face of limb, East.	
Sept. 15—3° 7′	9″,56	Sept. 7—3° 7′	2″,31
16	10 67	10	2,19
19	9,89	18	3,58
21	9,38	20	3,58
23	9,46	22	3,13
25	8,62	24	3,62
29	8,70	26	3,10
30	9,49	28	2,61
		Oct. 1	2,51
		2	3,79
Mean 3 7	9,47	Mean 3 7	3,04

Zenith dist. 3° 7′ 6″,25 + 3″,05 (refr. &c.) = 3° 7′ 9″,30.
Line of collimation 3,21.

51 *Draconis, N.*

Sept. 8—0° 52′	26″,01	Sept. 7—0° 52′	20″,13
16	27,89	10	23,30
19	27,60	18	21,23
23	25,70	20	20,26
25	25,98	22	20,06
29	24,59	26	20,05
30	27,43	28	19,87
		Oct. 1	21,01
		2	20,31
Mean 0 52	26,46	Mean 0 52	20,68

Zenith dist. 0° 52′ 23″,57 + 0″,85 (refr. &c.) = 0° 52′ 24″,42.
Line of collimation 2,89.

1 *κ Cygni, N.*

Sept. 8—0° 47′	4″,07	Sept. 7—0° 46′	57″,79
15	5,02	18	59,88
16	6,59	20	58,5
18	6,13	22 47	0,44
23	5,77	24 46	59,04
25	4,06	26	58,90
29	5,67	28	58,56
30	5,82	Oct. 1	59,13
		2	58,79
Mean 0 47	5,39	Mean 0 46	58,94

Zenith dist. 0° 47′ 2″,16 + 0″,76 (refr. &c.) = 0° 47′ 2″,92.
Line of collimation 3,22.

10 *ι Cygni.*

Sept. 19—0° 54′	35″,59	Sept. [illegible] 54′	40″,47
21	34,94	20	[illegible]
23	36,74	22	[illegible]
25	36,83	24	41,[illegible]
27	35,86	26	41,[illegible]
29	35,[illegible]	[illegible]	40,28
30	34,11	1	40,14
		2	40,24
Mean 0 54	[illegible],62	Mean 0 [illegible]	[illegible],39

Zenith dist. 0° 54′ 38″,21 + 0″,88 (refr. &c.) = 0° 54′ 39″,09.
Line of collimation [illegible],48.

γ *Ursæ*, N.

Face of limb, West.	Face of limb, East.	
Sept. 23—2° 34′ 12″,07	Sept. 18—2° 34′ 5″,73	Zenith dist. 2° 34′ 9″,59 + 2″,29 (refr. &c.) = 2° 34′ 11″,88.
26 15,28	5,29	Line of collimation 4,08.
Mean 2 34 13,67	Mean 2 34 5,51	

η *Ursæ*, S.

Sept. 10—1° 55′ 1″,14	Sept. 20—1° 55′ 4″,41	Zenith dist. 1° 55′ 3″,03 + 1″,65 (refr. &c.) = 1° 55′ 4″,68.
23 54 59,80	24 4,84	Line of collimation 2,79.
25 55 2,57	26 7,75	
30 54 57,42	28 5,68	
	Oct. 3 6,41	
Mean 1 55 0,23	Mean 1 55 5,82	

ζ *Ursæ*, N.

Sept. 26—3° 44′ 11″,63	Sept. 24—3° 44′ 6″,99	Zenith dist. 3° 44′ 8″,63 + 3″,73 (refr. &c.) = 3° 44′ 12″,36.
	30 4,67	Line of collimation 3,0.
	Oct. 3 5,28	
	Mean 3 44 5,64	

22 τ *Herculis*, S.

Sept. 5—5° 25′ 50″,78	Sept. 18—5° 25′ 58″,80	Zenith dist. 5° 25′ 54″,79 + 5″,03 (refr. &c.) = 5° 25′ 59″,82.
		Line of collimation 4,01.

α *Persei*, S.

Sept. 16—3° 4′ 26″,57	Sept. 8—3° 4′ 30″,66	Zenith dist. 3° 4′ 29″,53 + 3″,07 (refr. &c.) = 3° 4′ 32″,60.
18 27,69	12 31,49	Line of collimation 2,63.
23 26,22	19 32,95	
26 27,10	22 32,91	
	25 32,91	
Mean 3 4 26,89	Mean 3 40 32,18	

Capella, S.

Sept. 14—6° 26′ 12″,39	Sept. 11—6° 26′ 19″,64	Zenith dist. 6° 26′ 16″,46 + 6″,44 (refr. &c.) = 6° 26′ 22″,90.
16 13,75	12 18,89	Line of collimation 2,80.
19 13,59	18 21,00	
21 14,68	20 19,10	
[illegible]	[illegible]	

γ *Draconis, N.*

Face of limb, West.	Face of limb, East.	
April 16—0° 2′ 28″,37	April 22—0° 2′ 19″,05	Zenith dist. 0° 2′ 24″,36 + 0″,03 (refr. &c.) = 0° 2′ 24″,3
19 29,92	23 20,54	Line of collimation 4,57.
25 28,55		
Mean 0 2 28,94	Mean 0 2 19,79	

45 *d Draconis, N.*

April 19—5° 25′ 14″,51	April 23—5° 25′ 6″,17	Zenith dist. 5° 25′ 10″,22 + 5″,59 (refr. &c.) = 5° 25′ 15″,8
25 14,03		Line of collimation 4,05.
Mean 5 25 14,27		

46 *c Draconis, N.*

April 25—3° 51′ 57″,64.

51 *Draconis, N.*

April 19—1° 37′ 18″,23	April 23—1° 37′ 8″,79	Zenith dist. 1° 37′ 12″,61 + 1″,54 (refr. &c.) = 1° 37′ 14″,1
25 14,62		Line of collimation 3,81.
Mean 1 37 16,42		

1 κ *Cygni.*

April 25—1° 31′ 54″,14	April 23—1° 31′ 46″,65	Zenith dist. 1° 31′ 50″,39 + 1″,48 (refr. &c.) = 1° 31′ 51″,8
		Line of collimation 3,74.

10 ι *Cygni.*

April 19—0° 9′ 45″,02	April 23—0° 9′ 53″,90	Zenith dist. 0° 9′ 49″,41 + 0″,20 (refr. &c.) = 0° 9′ 49″,6
		Line of collimation 4,44.

γ *Ursæ.*

April 24—3° 19′ 7″,08	April 21—3° 18′ 55″,69	Zenith dist. 3° 19′ 1″,43 + 3″,24 (refr. &c.) = 3° 19′ 4″,6
	22 55,55	Line of collimation 5,65.
	23 56,12	
	Mean 3 18 55,78	

η *Ursæ.*

April 16—1° 10′ 10″,19	April 23—1° 10′ 17″,60	Zenith dist. 1° 10′ 13″,85 + 1″,22 (refr. &c.) = 1° 10′ 15″,0
		Line of collimation 3,70.

85 ι *Herculis.*

April 16—5° 20′ 20″,47	April 23—5° 20′ 32″,04	Zenith dist. 5° 20′ 25″,10 + 5″,61 (refr. &c.) = 5° 20′ 30″,7
19 15,87		Line of collimation 6,93.
Mean 5 20 18,17		

Capella.

April 13—5° 41′ 21″,09	April 24—5° 41′ [illegible]	Zenith dist. 5° 41′ 26″,4[illegible] (refr. &c.) = 5° 41′ [illegible]
21 22,74		
Mean 5 41 21,91		

Previous to my entering on the following article, it may not be improper to exhibit, under their proper points of view, the several quantities derived from observation, expressive of the differences of the zenith distances, or the deviation of the point of intersection of the meridional and horizontal wires from the true line of collimation.

At Dunnose.

	″
β Draconis - - - -	3,42
γ ——— - - - -	3,64
45 d ——— - - - -	4,46
46 c ——— - - - -	3,41
51 ——— - - - -	3,40
μ ——— - - - -	3,68
16 ——— - - - -	3,58
1 κ Cygni - - - -	3,80
10 ι ——— - - - -	3,15
γ Ursæ - - - -	3,02
η ——— - - - -	3,06
ζ ——— - - - -	3,76
85 ι Herculis - - - -	3,46
υ ——— - - - -	3,35
52 υ ——— - - - -	3,76
22 τ ——— - - - -	3,16
Capella - - - -	2,21

10	ι	Cygni	3,55
	η	Ursæ	3,67
	ζ	———	3,37
85	ι	Herculis	4,52
	υ	———	4,14
52		———	4,04
22	τ	———	4,16
	α	Persei	2,28
		Capella	5,30

At Arbury Hill.

	β	Draconis	2,71
	γ	———	2,92
45	d	———	3,65
46	c	———	3,21
51		———	2,89
1	κ	Cygni	3,22
10	ι	———	2,48
	γ	Ursæ	4,08
	η	———	2,79
	ζ	———	3,00
22	τ	———	4,01
	α	Persei	2,63
		Capella	2,89

At Greenwich.

	β	Draconis	4,07
	γ	———	4,57
45	d	———	4,05
51		———	3,81
1	κ	Cygni	3,74
10	ι	———	4,44
	γ	Ursæ	5,65
	η	———	3,70
		Capella	4,56

Amplitudes of the celestial Arc comprehended by the Stations Dunnose and Clifton.

β Draconis.

	°	′	″
nith distance at Dunnose	1	50	5,24
Ditto - Clifton -	1	0	17,84
Amplitude of arc - -	2	50	23,08

γ Draconis.

	°	′	″
nith distance at Dunnose	0	53	56,63
Clifton -	1	56	26,64
Amplitude of arc -	2	50	23,27

45 d Draconis.

	°	′	″
nith distance at Dunnose	6	16	47,66
Clifton -	3	26	22,92
Amplitude of arc -	2	50	24,74

51 Draconis.

	°	′	″
enith distance at Dunnose	2	28	44,05
Clifton -	0	21	38,12
Amplitude of arc -	2	50	22,17

46 c Draconis.

	°	′	″
enith distance at Dunnose	4	43	28,93
Clifton -	1	53	6,24
Amplitude of arc -	2	50	22,69

16 Draconis.

	°	′	″
enith distance at Dunnose	2	42	33,26
Clifton -	0	7	51,25
Amplitude of arc -	2	50	24,51

μ Draconis.

	°	′	″
nith distance at Dunnose	4	6	52,[illegible]

η Ursæ.

	°	′	″
Zenith distance at Dunnose	0	18	42,93
Clifton -	3	9	6,98
Amplitude of arc -	2	50	24,05

γ Ursæ.

	°	′	″
Zenith distance at Dunnose	4	10	36,23
Clifton -	1	20	13,53
Amplitude of arc -	2	50	22,70

ζ Ursæ.

	°	′	″
Zenith distance at Dunnose	5	20	35,66
Clifton -	2	30	10,37
Amplitude of arc -	2	50	25,29

59 Herculis.

	°	′	″
Zenith distance at Dunnose	4	17	1,28
Clifton -	7	7	25,45
Amplitude of arc -	2	50	24,17

85 ι Herculis.

	°	′	″
Zenith distance at Dunnose -	4	30	1,95
Clifton -	7	20	24,98
Amplitude of arc	2	50	23,03

υ Herculis.

	°	′	″
Zenith distance at Dunnose	4	1	33,24
Clifton -	6	51	56,80
Amplitude of arc -	2	50	23,56

22 τ Herculis.

	°	′	″
Zenith distance at Dunnose	3	49	37,1[illegible]

Amplitudes of the celestial Arc comprehended by the Stations Dunnose and Arbury Hill.

β Draconis.

	°	′	″
Zenith distance at Dunnose	1	50	5,24
Arbury Hill	0	13	45,82
Amplitude of arc -	1	36	19,42

γ Draconis.

	°	′	″
Zenith distance at Dunnose	0	53	56,63
Arbury Hill	0	42	22,73
Amplitude of arc -	1	36	19,36

45 *d Draconis.*

	°	′	″
Zenith distance at Dunnose	6	16	47,66
Arbury Hill	4	40	27,21
Amplitude of arc -	1	36	20,45

51 *Draconis.*

	°	′	″
Zenith distance at Dunnose	2	28	44,01
Arbury Hill	0	52	24,42
Amplitude of arc -	1	36	19,59

46 *c Draconis.*

	°	′	″
Zenith distance at Dunnose	4	43	28,93
Arbury Hill	3	7	9,30
Amplitude of arc -	1	36	19,63

1 *κ Cygni.*

	°	′	″
Zenith distance at Dunnose	2	23	22,86
Arbury Hill	0	47	2,92
Amplitude of arc -	1	36	19,94

10 *ι Cygni.*

	°	′	″
Zenith distance at Dunnose	0	41	40,68
Arbury Hill	0	54	39,09
Amplitude of arc -	1	36	19,77

η Ursæ.

	°	′	″
Zenith distance at Dunnose	0	18	42,93
Arbury Hill	1	55	4,63
Amplitude of arc -	1	36	21,70

Capella.

	°	′	″
Zenith distance at Dunnose	4	50	2,88
Arbury Hill	6	26	22,90
Amplitude of arc -	1	36	20,02

45 *d Draconis.*

	°	′	″
Zenith distance at Dunnose	6	16	47,66
Greenwich	5	25	15,81
Difference of latitude	0	51	31,85

51 *Draconis.*

	°	′	″
Zenith distance at Dunnose	2	28	44,05
Greenwich	1	37	14,25
Difference of latitude	0	51	29,90

1 *ϰ Cygni.*

	°	′	″
Zenith distance at Dunnose	2	23	22,86
Greenwich	1	31	51,87
Difference of latitude	0	51	30,99

10 *ι Cygni.*

	°	′	″
Zenith distance at Dunnose	0	41	40,68
Greenwich	0	9	49,60
Difference of latitude	0	51	30,28

γ Ursæ.

	°	′	″
Zenith distance at Dunnose	4	10	36,23
Greenwich	3	19	4,67
Difference of latitude	0	51	31,56

η Ursæ.

	°	′	″
Zenith distance at Dunnose	0	18	42,93
Greenwich	1	10	15,07
Difference of latitude	0	51	32,14

It will now be proper to exhibit the various results, as previously deduced; the amplitudes of the several arcs will then stand as follow.

Arc between Dunnose and Clifton.

	°	′	″	
β Draconis - - -	2	50	23,08	
γ ——— - - -			23,27	
45 d ——— - - -			24,75	
46 c ——— - - -			22,69	
51 ——— - - -			22,17	
16 ——— - - -			24,51	
μ ——— - - -			21,10	Extreme results. Mean 23″,19, and might be rejected
ζ Ursæ - - -			25,29	
η ——— - - -			22,70	

Between Dunnose and Arbury Hill.

β	Draconis - -	1° 36′ 19″,42
γ	——— - - -	19,36
45 d	——— - - -	20,45
46 c	——— - - -	19,63
51	——— - - - -	19,59
1 κ	Cygni - - - -	19,94
10 ι	——— - - - -	19,77
η	Ursæ - - - -	21,70
Mean amplitude	- -	1 36 19,98

Between Dunnose and Greenwich.

β	Draconis - -	0° 51′ 32″,11
γ	——— - - - -	32,24
45 d	——— - - -	31,85
51	——— - - -	29,90
1 κ	Cygni - - - -	30,99
10 ι	——— - - -	30,28
γ	Ursæ - - - -	31,56
η	——— - - - -	32,14
Mean amplitude	- -	0 51 31,39

It is very generally known that his Grace the Duke of MARLBOROUGH is possessed of an excellent quadrant, made by the late Mr. RAMSDEN, and that he has for some years been in the habit of using it at Blenheim. As my meridional line is not far eastward from his Grace's observatory, the zenith distance of any star or stars there determined, from a course of accurate observations, must afford me the means of ascertaining the lengths of the degrees on the meridian, at the middle points between Blenheim and the two extremities of my arc. I therefore applied to his Grace, requesting him to favour me with any observations he might have made, and with permission to publish

tem, if I thought proper. His Grace was pleased to comply with my request; and I now avail myself of the advantage procured by that condescension.

Blenheim Observatory.

Zenith Distances of γ Draconis, reduced to the Beginning of the Year 1794, *from Observations made in five successive Years, by his Grace the Duke of* MARLBOROUGH.

From the observations of	1794,	0° 19′ 17″,32	γ Draconis south of the zenith.
	1795	17,70	
	1796	17,51	
	1797	17,48	
	1798	17,32	
	Mean	0 19 17,46.	

Therefore, the mean zenith distance of γ Draconis, at Blenheim, on the 1st of January, 1802, may be taken at 0° 19′ 23″,06 south. The zenith distance of this star, at the same period, at the station Dunnose, as derived from the late operation, is 0° 53′ 56″,63 north; therefore, 0° 53′ 56″,63 + 0° 19′ 23″,06 = 1° 13′ 19″,69, is the difference of latitude between Dunnose and Blenheim observatory; and here, perhaps, it may not be improper to advert to page 675 of the Phil. Trans. for 1800, where the observed and computed latitudes are given, the former being 51° 50′ 24″,9, and the latter 51° 50′ 28″,1. The latitude of Dunnose is 50° 37′ 8″,21, that of Greenwich being taken at 51° 28′ 40″; [illegible] 0° 51′ [illegible], as derived from the observations made with the [illegible]. Hence, 50° 37′ 8″,21 +

shall conclude this article with giving, in order, the subtenses in the heavens, of the different parts of my terrestrial arc.

1.	Dunnose and Clifton - -	2° 50′ 23″,38
2.	Dunnose and Arbury Hill -	1 36 19,98
3.	Arbury Hill and Clifton - -	1 14 3,40
4.	Dunnose and Greenwich -	0 51 31,39
5.	Greenwich and Clifton - -	1 58 51,59
6.	Arbury Hill and Greenwich -	0 44 48,19
7.	Dunnose and Blenheim -	1 13 19,69
8.	Blenheim and Clifton - -	1 37 3,69

Determination of the Lengths of the Degrees on the Meridian, in the middle Points of the several Arcs given in the last Article.

On a reference to the Phil. Trans. for 1800, it will be found, that Blenheim Observatory is 446458 feet from the perpendicular to the meridian of Dunnose. But the parallel to the perpendicular at Dunnose, from that observatory, where it cuts the meridian of the former, is about $\frac{4}{10}$ of a second in latitude north of the latter; therefore, 446498 feet may be taken for the distance of Blenheim north of Dunnose. This premised, we have the following terrestrial arcs, in conjunction with the preceding celestial ones, for computing the lengths of the several degrees.

	Arcs.	Feet.
1.	Dunnose and Clifton - - -	1036337
2.	Dunnose and Arbury Hill - -	586320
3.	Arbury Hill and Clifton - -	450017
4.	Dunnose and Greenwich - -	313696
5.	Greenwich and Clifton - -	722641
6.	Arbury Hill and Greenwich - -	[illegible]
7.	Dunnose and Blenheim - -	446498
8.	Blenheim and Clifton - -	[illegible]

And, by simply dividing the terrestrial arcs by their corresponding celestial ones, and afterwards multiplying the several quotients by 3600″, we shall get the lengths of the degrees as follows.

		Fathoms.
Middle point between	Dunnose and Clifton -	60820
	Dunnose and Arbury Hill -	60864
	Arbury Hill and Clifton -	60766
	Dunnose and Greenwich -	60884
	Greenwich and Clifton -	60794
	Arbury Hill and Greenwich	60849
	Blenheim and Clifton -	60769
	Blenheim and Dunnose -	60890

Taking the latitude of Greenwich at 51° 28′ 40″, from the several arcs now given, the latitudes of their middle points are easily found; and, with the lengths of the degrees, when properly arranged, will stand as follows.

	Latitude of middle point.	Fathoms.
Arbury Hill and Clifton -	52° 50′ 29″,8	60766
Blenheim and Clifton - -	52 38 56,1	60769
Greenwich and Clifton -	52 28 5,7	60794
Dunnose and Clifton - -	52 2 19,8	60820
Arbury Hill and Greenwich	51 51 4,1	60849
Dunnose and Arbury Hill -	51 35 18,2	60864
Blenheim and Dunnose -	51 13 18,2	60890
Dunnose and Greenwich -	51 2 54,2	60884

Note. The altitude of Arbury Hill, above the level of the sea, is 804 feet. The altitudes of the stations southward of Arbury Hill, are given in the former accounts of the trigonometrical operations: those to the northward of Arbury Hill may be found from the following *data*.

At Sutton, Heatherseedge, elev. 15′ 25″; Gringley, dep. 18′ 47″.—*At Castle Ring*, Orpit Heights, dep. 5′ 26″; Bardon Hill, dep. 6′ 48″; Corley, dep. 14′ 26″.—*At Heatherseedge*, Orpit Heights, dep. 20′ 27″.—*At Clifton*, Heatherseedge, elev. 29′ 12″; Gringley, dep. 13′ 40″.—*At Hollan Hill*, Bardon Hill, elev 2′ 35″; Orpit Heights, elev. 12′ 0″; Sutton, elev. 7′ 12″.—*At Bardon Hill*, Corley, dep. 16′ 3″; Arbury Hill dep. 16′ 0″; Castle Ring, dep. 12′ 30″; Sutton, dep. 19′ 48″; Orpit Heights, dep. 6′ 35″

CONCLUSION.

From this measurement it appears, that the length of a degree on the meridian, in latitude 52° 2′ 20″, is 60820 fathoms. This conclusion is deduced from the supposition of the whole arc subtending an angle of 2° 50′ 23″,38 in the heavens, and a distance of 1036337 feet on the surface of the earth.

The length of the degree at the middle point (51° 35′ 18″) between the southern extremity of the arc and Arbury Hill, is 60864 fathoms; which is greater than the above, and exceeds it by 44 fathoms. But this degree, admitting the earth to be an ellipsoid, with the ratio of its axes as 229 to 230, should be about 10 fathoms less. If the measurement of the terrestrial arc be sufficiently correct, and the earth of an elliptical form in these latitudes, either the arcs affording the deductions are incorrect, or some material deflection of the plumb-line has taken place, at one or two stations, from the effect of attraction.

Without arrogating to myself any merit from the pains taken in the performance of this undertaking, I may say, I am so perfectly convinced of the general accuracy of the whole, that I cannot for a moment doubt the collective evidence of its sufficiency. From an examination of my field books, and from the remeasurement of the chains used in our base-line on Misterton Carr, I think it is probable that an error in the whole distance, of 197 miles nearly, does not subsist to an amount of more than 100 feet, corresponding to 1″ in the amplitude of the whole arc; and I also think it probable it cannot amount to half that quantity. The supposition of the zenith distances of the stars being generally erroneous, at any one station, cannot be admitted, unless it should be imagined, that the plane of the

ector's limb was not got into that of the meridian. Such an idea, however, can scarcely be entertained, after a careful examination of the several observations, and a due attention to the neans by which the instrument was made to assume its right osition. Perhaps, also, I should not fail to observe, in this lace, that although the instrument was always brought into the lane of each meridian by means of the telescope attached to he side of the great tube, and the azimuth circle, yet, having wo good chronometers in my possession, I repeatedly verified he truth of the sector's position, by observing the transits of wo stars, north and south of the zenith, at the greatest distances ny arc would admit of. But, to return, if there be an error in the mplitude of the total arc, from a deflection of the plumb-line at either of the stations, it is not probable that any such deflection existed at Dunnose; as the deviation of it towards the north, rom a deficiency of matter towards the channel, would tend to liminish the inequality between the lengths of the two degrees. This will be evident, on consideration. I am therefore disposed o believe that the plumb-line was drawn towards the south, rom the action of matter, both at the northern extremity of the arc and at Arbury Hill, but more particularly at the first-mentioned station. If this were partly the case, and both Dunnose and Arbury Hill were free from any such prevailing cause, the total arc must be too great, if taken at 2° 50′ 23″,38, by about 8″, nearly answering to 2″ on each degree. A deviation of 8″ from the true vertical, is a large quantity; nor can the cause of it be assigned, unless it be also supposed, that the matter producing that deflection extends in a southern direction *beyond* Arbury Hill. If the error, though not probable, as above observed, be supposed to exist at Dunnose, it must amount to

nore than 10″; and that too from the effects of attraction in a outhern direction, where the deficiency of matter would lead is to believe the reverse would happen.

I am perfectly aware that it is possible to state a case, in which the plumb-line of a sector would deviate from the true vertical by such a quantity. Thus, for instance, in a chalky county, like the southern part of the kingdom, if the instrument were set up adjoining the terminations of two strata running east and west, one of chalk and the other of much denser materials, the effect would be as we have found it. But, at Dunnose, this argument does not apply; nor is there reason to believe, from external appearances, that it will do so, with regard either to Arbury Hill or the northern extremity of the meridional line.

It was the discovery of the disagreement between the subtense in the heavens, of the whole arc, and its corresponding terrestrial one, with those of its parts, which led me to apply to his Grace the Duke of MARLBOROUGH, for the observations made at Blenheim on γ Draconis, or some other star. His Lordship's compliance with my request, is shown, from the Table of results, to be serviceable; as the arc contained between the observatory at Blenheim and Dunnose, deduced from his Grace's observations, and those made at the latter place, with the meridional distance 446498 feet, give 60890 fathoms, for the length of the degree on the meridian in latitude 51° 13′; which agrees nearly with the length of the degree at the middle point between Greenwich and Dunnose. However, under all considerations of the means by which the degree in 51° 13′ has been obtained, I am inclined to believe there is an uncertainty in it, of 6 or 7 fathoms, answering to about ½″ in latitude.

But, if the measured space between his Grace's observatory and

Dunnose, with its amplitude, ($1° 13' 19'',69$) be used in finding the meridional distance of the whole arc, (its corresponding amplitude,) we shall get $2° 50' 11'',80$ for its subtense; which argues a deflection from the vertical at Clifton $= 11'',79$. If the meridional distance between Dunnose and Greenwich be used, we shall, from the same mode of proceeding, make it $= 10'',3$. In short, the general tenor of the observations seems to prove, that the plumb-line of the sector has been drawn towards the south at all the stations; and that by attractive forces, which increase as we proceed northward. On a further prosecution of this Survey, the zenith sector will be taken forward in that direction, which will afford an opportunity of throwing further light on this interesting subject. But meridional operations carried on in insular countries, are not so likely to afford just conclusions with regard to the different lengths of the degrees, as the same operations conducted in places very remote from deep seas.

From the late operations of the French Academicians it appears, that the meridional distance between Dunkirk and Barcelona is 275792,36 modules, the metre being 443,296 lines of the Peru toise $= 0,256537$th part of the module, at the temperature of melting ice. This meridional distance, therefore, converted into English feet, is 3527921. The distance between Dunkirk and Paris is 133758 feet, and the distance between Paris and Greenwich $= 963954$ feet; therefore, 830196 feet is the distance between Greenwich and Dunkirk. The distance between Greenwich and Clifton is 722641 feet; hence, 4411968 feet is the meridional distance between Clifton and Barcelona. The latitude of Barcelona is $41° 21' 48'',8$; the latitude of Greenwich is $51° 28' 40''$; and if to this latitude we add $1° 58' 51'',59$, the arc between Clifton and Greenwich, we shall get

53° 27′ 31″,59 for the latitude of Clifton; and shall then have the difference of latitude between Barcelona and Clifton = 12° 5′ 42″,79, something more than the 30th part of the whole circumference of the earth. With this difference of latitude, and the abovementioned distance, we shall get 60795 fathoms, for the mean length of a degree on the earth's surface, in latitude 47° 24′. The latitude of Paris is 48° 50′ 15″; this, with that of Clifton, gives 4° 37′ 16″,59 for the difference between their parallels. The meridional distance is 1686595 feet; hence, 60825 fathoms, is the length of the degree in latitude 51° 9′.

With regard to the latitudes of places published in our former papers, those referred to the meridian of Greenwich are to remain uncorrected, since the computations were made with nearly the same length of a degree on the meridian, as that at the middle point, now deduced, between Dunnose and Greenwich, *viz.* 60884 fathoms. As to those places referred to the new meridian, *viz.* Dunnose, Butterton, and St. Agnes Beacon, 1″ is to be added to the latitudes of them all; because the latitude of Dunnose became the standard, which was then computed to be 50° 37′ 7″,3, but is now found, from the zenith distances of the stars observed there and at Greenwich, to be 50° 37′ 8″,2.

By way of Appendix to this Paper, I shall subjoin the latitudes and longitudes of those places intersected in the survey of Essex, Suffolk, &c. whose distances from their respective places of observation are given in the Phil. Trans. for 1800; this cannot but be highly useful, as they may be depended on, the interior survey of those parts having since proved that no erroneous intersections were made.

APPENDIX.

Bearings of the principal Stations in the Counties of Essex, &c. from the Parallels to the Meridian of Greenwich; and likewise their Distances from that Meridian.

Names of stations.		Bearings. °	′	″		Distance from meridian. Feet.	Mean. Feet.		Distance from perpendicular Feet.	Mean. Feet.
Severndroog	New Station, Wrotham	46	27	19	SE	71978	71977	E	59145	59144½ S
Old Station, Wrotham -		38	43	11	NE	71976			59144	
Severndroog	Gravesend	80	53	20	SE	84888	84888½	E	15434	15433½ S
New Station, Wrotham		16	27	19	NE	84889			15433	
Severndroog	Langdon Hill	68	48	4	NE	96515	96515	E	27920	27920½ N
Gravesend -		15	0	39	NE	96515			27921	
Gravesend -	Hadleigh	49	32	32	NE	133643	133643½	E	26145	26145 N
Langdon Hill		87	15	37	SE	133644			26145	
Hadleigh -	Halstow	7	45	59	SW	129041	129040	E	7607	7607 S
Gravesend -		79	56	53	NE	129039			7607	
Gravesend -	Gads Hill	68	24	46	SE	105603	105603	E	23629	23629 S
Halstow -		55	38	32	SW	105603			23629	
Gad's Hill -	Sheppey	88	46	1	NE	176273	176273	E	22108	22108½ S
Halstow -		72	55	56	SE	176273			22109	
Hadleigh -	South End	8[illegible]	11	2	SE	160836	160836	E	21441	21441 N
Sheppey -		19	31	7	NW	160836			21441	
Halstow - -	Rayleigh	11	17	7	NE	138709	138709½	E	40851	40850½ N
Sheppey -		30	19	17	NW	138710			40850	
Halstow -	Prittlewell	42	47	6	NE	160844	160844	E	26757	26756½ N
Sheppey -		17	31	22	NW	160844			26756	
Halstow -	Canewdon	33	18	22	SE	108413	108413	E	51908	51907½ N
Sheppey -		0	16	7	NW	108413			51907	
Halstow -	Flagstaff, Sheerness	86	3	4[illegible]	SE	170376	170374½	E	10451	10449 S
Hadleigh -		4[illegible]	6	25	SE	170373			10447	
Rayleigh -	Danbury	9	55	10	NW	130525	130528½	E	87654	87664 N
Frierning -		71	57	58	NE	130531			87664	
Langdon Hill		29	39	21	NE	130530			87662	
Severndroog	Frierning	42	23	31	NE	83919	83919	E	72488	72487½ N
Langdon Hill		15	46	56	NE	83919			72487	
Rayleigh -	Signal Staff, Shoeburyness	59	55	0	SE	179732	179734	E	17086	17089 N
Langdon Hill		82	35	14	SE	179736			17093	
Rayleigh -	Old Station, Tiptree	13	45	9	NE	156314	156314	E	112779	112785 N
Danbury -		45	44	2	NE	156310			112790	
Frierning -		60	53	59	NE	156320			112786	
Tiptree* -	Tillingham Steeple	54	44	17	SE	200547	200544½	E	81511	81514 N
Danbury -		84	58	57	SE	200542			81518	

* Triptree, by mistake, in the former part of this Survey.

Names of stations.		Bearings.	Distance from meridian.	Mean.		Distance from perpendicular	Mean.	
		° ′ ″	Feet.	Feet.		Feet.	Feet.	
Danbury -	Peldon	62 33 13 NE	200463	200464	E	123986	123984	N
Tillingham -		0 6 23 NW	200465			123983		
Peldon -	Flagstaff, St. Osyth Priory	83 48 39 SE	243805	243808	E	119284	119283	N
Tillingham -		48 52 57 NE	243812			119282		
Danbury -	Great Tey Steeple	30 12 45 NE	169380	169381	E	154381	154381	N
Peldon - -		45 38 20 NW	169382			154381		
Peldon -	Stoke Steeple	0 35 41 NE	201127	201127	E	187921	187921	N
Great Tey -		43 25 34 NE	201127			187921		
Peldon -	Thorp Steeple	75 21 46 NE	263163	263163	E	140362	140359	N
Stoke - -		52 30 43 SE	263164			140358		
Peldon -	Little Bentley	60 4 57 NE	244846	244846	E	149523	149523	N
Thorp - -		63 25 21 NW	244846			149523		
Little Bentley	Dover Court	63 32 9 NE	283323	283322	E	168677	168676	N
Thorp -		35 26 59 NE	283321			168675		
Tillingham -	West Mersea	11 58 27 NE	206545	206544	E	109809	109810	N
Danbury -		73 45 20 NE	206543			109811		
Great Tey	St. Mary's, Colchester	84 22 42 SE	202276	202276	E	151143	151143	N
Stoke - -		1 47 26 SE	202276			151143		
St. Mary's Colchester -	Little Bromley	76 2 46 NE	234987	234987	E	159270	159270	N
Stoke -		49 45 52 SE	234988			159270		
Thorp - -	Tattingstone	14 37 54 NW	250358	250353	E	189406	189402	N
Dover Court		57 50 38 NW	250351			189405		
Stoke -		88 17 13 NE	250350			189393		
Tattingstone	Rushmere	35 36 22 NE	270865	270864	E	218047	218048	N
Dover Court		14 9 47 NW	270864			218050		
Rushmere -	Falkenham	52 35 7 SE	302055	302054	E	194188	194189	N
Dover Court		36 17 7 NE	302054			194190		
Rushmere -	Woodbridge	69 24 43 NE	295524	295524	E	227311	227311	N
Falkenham		11 9 17 NW	295524			227312		
Woodbridge	Butley Steeple	85 40 43 NE	329485	329485	E	229878	229878	N
Falkenham -		37 32 43 NE	329485			229879		
Falkenham	Light House, Orford	59 30 44 NE	354267	354266	E	224929	224929	N
Butley -		78 42 16 SE	354266			224931		
Rushmere -	Otley Steeple	6 39 43 NE	274256	274254	E	247089	247088	N
Woodbridge		47 5 17 NW	274252			247087		
Otley - -	Henley Steeple	53 4 43 SW	259074	259075	E	235681	235681	N
Rushmere -		33 45 47 NW	259076			235681		
Henley -	Copdock Steeple	23 3 13 SW	245556	245557	E	203918	203917	N
Rushmere -		60 49 13 SW	245559			203917		
Copdock -	Naughton Steeple	51 26 57 NW	214045	214045	E	229030	229030	N
Henley - -		81 35 53 SW	214046			229030		
Great Tey -	Twinstead Steeple	12 47 14 NW	161215	161206	E	161198	161198	N
Stoke - -		86 26 30 NW	161198			161198		
Stoke -	Lavenham	28 18 20 NW	178348	178348	E	230216	230216	N
Glemsford -		87 4 35 NE	178348			230216		
Stoke -	Bulmer	73 17 13 NW	154916	154915	E	201792	201792	N
Lavenham -		39 30 10 SW	154915			201793		

Names of stations		Bearings.	Distance from meridian.	Mean.		Distance from perpendicular.	Mean.	
		° ′ ″	Feet.	Feet.		Feet.	Feet.	
Lavenham -	Glemsford	87 4 35 SW	152635	152636	E	228903	228903	N
Bulmer - -		4 48 30 NW	152636			228903		
Bulmer -	Toppesfield *	/7 14 50 SW	120796	120796	E	194070	19 069	N
Lavenham -		57 52 10 SW	120796			194069		
Severndroog	Southweald	39 49 54 NE	61300	61298	E	52599	52596	N
Langdon Hill		54 59 11 NW	61296			52593		
Tiptree -	Gallywood Common	57 45 6 SW	120796	120796	E	80657	80658	N
Danbury -		74 25 56 SW	120796			80659		
Gallywood Common - -	Pleshey *	17 28 50 NW	93384	93385	E	118789	118787	N
Tiptree -		84 33 10 NW	93386			118786		
Gallywood Common -	High Easter	33 14 20 NW	79208	79208	E	120610	120611	N
Pleshey -		82 39 50 NW	79209			120612		
Danbury -	Hatfield Oak	62 7 31 NW	55309	55306	E	127468	127466	N
Pleshey -		77 9 51 NW	55303			127464		
Pleshey - -	Beauchamp Roding	63 25 9 SW	64941	64940	E	104555	104555	N
Hatfield Oak		22 49 0 SE	64940			104556		
Danbury -	Thaxted	31 33 51 NW	77490	77481	E	174002	173995	N
Lavenham -		60 52 10 SW	77475			174000		
Stoke - -		83 34 10 SW	77480			173985		
Severndroog	Brentwood Spire	44 23 29 NE	68984	68984	E	52063	52061	N
Langdon Hill		48 45 17 NW	68984			52060		
St. Paul's -	Old Station, High Beech	29 8 28 NE	8117	8117	E	67219	67219	N
Severndroog		4 44 36 NW	8117			67219		
St. Paul's -	Station, Hampstead	43 14 15 NW	39822	39824	W	32055	32055	N
High Beech -		53 44 33 SW	39826			32056		
St. Paul's -	New Station, High Beech	28 45 2 NE	7661	7661	E	67265	67264	N
Old Station, High Beech -		84 18 44 NW	7661			67264		
High Beech -	Epping Mill	53 4 40 NE	21712	21742	E	77457	77457	N
Severndroog		5 24 9 NE	21742			77457		
High Beech -	Berkhampstead Gazebo	46 15 36 NW	27987	27990	W	101790	101788	N
Epping Mill		63 56 1 NW	27993			101786		
Hatfield Oak -	Nasing Steeple	55 36 7 SW	10875	10875	E	97046	97046	N
Hatfield Oak	Henham on the Mount	0 35 42 NE	55710	55701	E	166727	166701	N
Thaxted -		71 24 58 SW	55692			166674		
Henham -	Chorley Steeple	36 40 8 SW	32376	32366	E	135373	135377	N
Hatfield -		70 57 48 NW	32356			135381		
Henham - -	Elmdon	35 5 52 NW	29669	29664	E	203744	203730	N
Thaxted -		58 8 32 NW	29660			203717		
Elmdon -	Rickling	22 34 52 SE	40925	40928	E	176651	176657	N
Henham -		55 59 52 NW	40931			176664		
Elmdon - -	Albury	11 37 43 SW	19669	19660	E	155071	155072	N
Henham -		72 6 30 SW	19651			155073		
Elmdon -	Balsham	49 57 18 NE	71969	71969	E	239285	239278	N
Thaxted -		4 49 38 NW	71970			239272		

* Topplesfield, * Pleshley, } by mistake, in the former part of this Survey.

Names of stations.		Bearings	Distance from meridian.	Mean.	Distance from perpendicular.	Mean.
		° ′ ″	Feet.	Feet.	Feet.	Feet.
Elmdon -	Babraham Mount	26 18 32 NE	48970	48976 E	242777	242770 N
Balsham -		81 22 4 NW	48982		242762	
Elmdon -	Triplow	13 27 58 NW	23888	23891 E	227854	227850 N
Babraham Mount		59 15 2 SW	23894		227846	
Langdon Hill	Hornchurch	87 1 36 NW	51738	51747 E	30245	30246 N
Severndroog		47 41 55 NE	51744		30248	
Gravesend -	Purfleet Cliff	60 30 51 NW	53972	53974 E	2048	2050 N
Hornchurch -		4 31 59 SE	53976		2050	
Severndroog	Barking	7 57 53 NE	17541	17544 E	20068	21069 N
Hornchurch -		74 58 39 SW	17547		21070	
St. Paul's -	Westham	71 14 54 NE	2265	2264 E	21387	21387 N
Severndroog		24 48 33 NW	2264		21388	

Bearings of secondary Objects, &c.

Severndroog	Chigwell	4 30 54 NE	18578	18581 E	53508	53510 N
Highbeech -		37 21 14 SE	18585		53513	
Severndroog	Billericay Chapel	54 20 37 NE	95374	95373 E	54286	54286 N
Frierning -		32 10 59 SE	95373		54286	
Hornchurch	Chimney of Public House	60 27 19 SW	22826	22821 E	13857	13855 N
Barking -		36 9 21 SE	22817		13853	
Purfleet Cliff	Rainham Steeple	38 43 29 NW	43722	43723 E	14835	14835 N
Hornchurch -		27 29 1 SW	43725		14836	
Hornchurch	Belvidere	27 18 51 SW	37806	37807 E	3263	3266 N
Purfleet -		85 40 59 NW	37808		3270	
Hornchurch -	Valence Tree	89 28 19 NW	31338	31335 E	30434	30432 N
Purfleet -		38 34 59 NW	31332		30430	
Rainham -	Cold Harbour	3 1 31 SW	43138	43137 E	3762	3761 N
Purfleet -		81 1 59 NW	43137		3760	
Gravesend -	Chadwell Steeple	1 13 50 NW	84524	84524 E	1570	1570 N
Severndroog		85 25 30 NE	84524		1571	
Gravesend -	Greys Steeple	36 52 50 NW	73799	73799 E	653	653 S
Chadwell -		78 17 30 SW	73799		653	
Gravesend -	Flagstaff, Mr. Button's	38 59 50 NW	70491	70491 E	2348	2347 N
Chadwell -		86 49 50 NW	70491		2347	
Gravesend -	West Thurrock Steeple	52 56 50 NW	66967	66967 E	1902	1902 S
Chadwell -		78 48 40 SW	66967		1903	
Gravesend -	West Tilbury Steeple	17 38 10 NE	89420	89420 E	1181	1181 S
Chadwell -		60 40 20 SE	89421		1181	
Gravesend -	Northfleet Steeple	70 45 17 NW	76023	76623 E	12548	12548 S
Chadwell -		29 13 52 SW	76623		12549	
Gravesend -	Horndon Spire	13 10 9 NE	92494	92496 E	17070	17072 N
Hornchurch -		72 5 26 SE	92498		17074	
Gravesend -	Flagstaff, East Tilbury	56 2 10 NE	98431	98431 E	6312	6312 S
Chadwell -		60 27 20 SE	98432		6313	
Gravesend -	Fobbing Steeple	34 47 40 NE	108532	108534 E	18591	18591 N
Halstow -		38 2 57 NW	108535		18592	

Names of stations.		Bearings.				Distance from meridian.	Mean.		Distance from perpendicular.	Mean	
		°	′	″		Feet.	Feet.		Feet.	Feet.	
Halstow - -	Thundersley Steeple	1	36	20	NE	—	130198	E	—	33719	N
Sheppey -	Leigh Steeple	30	29	48	NW	149211	149211	E	23840	23839	N
Halstow -		32	40	43	NE	149211			23838		
Prittlewell -	Little Wakering Steeple	76	58	11	NE	179943	179940	E	31175	31176	N
Canewdon -		29	38	19	SE	179937			31178		
Prittlewell -	Bank Flagstaff	83	58	11	NE	192575	192572	E	30108	30109	N
Canewdon -		48	15	19	SE	192570			30110		
Bank Flagstaff	Foulness Chapel	33	4	41	NE	203200	203199	E	46426	46426	N
Canewdon -		81	6	49	SE	203198			46427		
Tillingham -	Tillingham Grange Signal Staff	40	45	13	SE	209676	209676	E	70916	70917	N
Peldon - -		9	50	52	SE	209676			70918		
Tillingham -	Bradwell Point Signal Staff	43	21	35	NE	213453	213453	E	95184	95185	N
Peldon -		24	16	41	SE	213453			95187		
Tillingham -	Brightlingsea Steeple	30	56	17	NE	229383	229382	E	129627	129627	N
Peldon -		78	57	17	NE	229381			129628		
Tillingham -	Toleshunt Major	47	39	58	NW	173531	173531	E	106122	106123	N
Peldon -		56	27	2	SW	173531			106124		
Tillingham -	Tolesbury Steeple	27	50	13	NW	189052	189051	E	103277	103278	N
West Mersea		69	31	40	SW	189050			103279		
Tillingham -	Althorn Steeple	61	20	37	SW	172511	172512	E	66194	66192	N
Tiptree -		19	10	14	SE	172513			66191		
Althorn -	Burnham Steeple	62	50	23	SE	185324	185324	E	59619	59620	N
Tillingham -		34	48	27	SW	185324			59621		
Langdon Hill	Rettenden Steeple	45	18	38	NE	126748	126747	E	57827	57822	N
Rayleigh -		35	11	19	NW	126747			57817		
Langdon Hill	Runwell Steeple	44	47	13	NE	121154	121151	E	52743	52740	N
Rayleigh -		55	54	27	NW	121148			52737		
Rayleigh -	Great Burstead Steeple *	82	34	27	NW	96984	96987	E	46288	46293	N
Danbury -		39	2	12	SW	96990			46299		
Gallywood Common -	East Hanningfield Steeple	63	53	54	SE	125885	125884	E	70617	7[illegible]18	N
Danbury -		15	14	49	SW	125884			70619		
Canewdon -	Hockley Steeple	82	24	42	SW	144794	144796	E	48798	48798	N
Danbury -		20	9	38	SE	144799			48798		
Rettenden -	Stow, St. Mary's	63	26	42	NE	147833	147836	E	68360	63358	N
Canewdon -		50	59	18	NW	147839			68357		
Friefhing -	Stock Steeple	71	54	14	SE	99912	99912	E	67262	67261	N
Danbury -		56	19	22	SW	99912			67261		
Tiptree -	Southminster Steeple	36	6	6	SE	188765	188762	E	68286	68289	N
Tillingham -		41	42	29	SW	188760			68292		
Peldon -	Layer Marney Steeple	82	30	52	NW	180456	180457	E	126613	126612	N
Tillingham -		24	0	27	NW	180458			126612		
Peldon -	St Osyth Point Signal Staff	80	26	29	SE	260322	260323	E	113906	113905	N
Tillingham		61	33	1	NE	260324			113904		
Thorp [illegible]	Great Clackton Sig. Staff	26	32	57	SE	272780	272780	E	1[illegible]112	121111	N
Little [illegible]		44	30	52	SE	272780			[illegible]11		

* Great Burghsted, by mistake, in the former part of this Survey.

Names of stations.		Bearings.	Distance from meridian.	Mean.	Distance from perpendicular.	Mean.
		° ′ ″	Feet.	Feet.	Feet.	Feet.
Dover Court	Frinton *	10 50 11 SW	275991	275991 E	130375	130375 N
Thorp Steeple	Steeple	52 6 20 SE	275992		130375	
Thorp -	Great Clack-	3 45 53 SW	261921	261921 E	121481	121481 N
Peldon - -	ton Steeple	87 40 1 SE	261921		121481	
Dover Court -	Frinton Signal	3 49 35 SE	285637	285638 E	134068	134068 N
Thorp -	Staff	74 21 45 SE	285639		134068	
Dover Court	Walton Tower	17 48 27 SE	291358	291358 E	143661	143661 N
Thorp -		83 19 21 NE	291359		143661	
Dover Court	Cupola, Lan-	81 29 29 NE	298242	298242 E	170909	170909 N
Thorp - -	guard Fort	48 56 56 NE	298243		170909	
Thorp -	Ardleigh	58 21 57 NW	222726	222726 E	165270	165267 N
Peldon	Steeple	28 20 12 NE	222726		165265	
Great Tey -	Frating	78 10 31 SE	231313	231313 E	141415	141414 N
Peldon -	Steeple	60 31 56 NE	231313		141414	
Thorp -	Thorrington	86 16 44 SW	239323	239323 E	138810	138809 N
Little Bentley	Steeple	27 16 2 SW	239324		138809	
Thorp -	Kirby Steeple	84 44 24 SE	276355	276354 E	139146	139146 N
Dover Court -		13 16 47 SW	276353		139146	
Dover Court	Brantham	74 38 51 NW	242253	242254 E	179953	179951 N
Tattingstone	Steeple	40 35 22 SW	242255		179950	
Dover Court	Harwich	58 39 3 NE	290907	290909 E	173297	173297 N
Rushmere -	Steeple	24 7 47 SE	290911		173297	
Kirby Steeple	Little Oakley	5 5 13 NW	274475	274474 E	160255	160255 N
Dover Court		46 24 59 SW	274474		160255	
Dover Court -	Bawdsey	51 49 28 NE	319624	319624 E	197218	197217 N
Rushmere -	Steeple	66 51 57 SE	319625		197216	
Dover Court	Harkstead	45 2 45 NW	269208	269208 E	182770	182769 N
Rushmere -	Steeple	2 41 15 SW	269209		182768	
Dover Court -	Arwarton	24 33 8 NW	277899	277902 E	180550	180547 N
Tattingstone	Steeple	72 10 38 SE	277905		180544	
Tattingstone	Bradfield	6 0 38 SE	252552	252551 E	168520	168521 N
Arwarton -	Steeple	64 37 22 SW	252550		168523	
Falkenham -	Orford Steeple	51 16 53 NE	345336	345342 E	228887	228881 N
Rushmere -		81 43 43 NE	345349		228876	
Falkenham	Nacton Steeple	83 34 7 NW	277115	277115 E	197000	196998 N
Rushmere -		16 32 17 SE	277115		196997	
Dover Court	Capel Steeple	63 18 59 NW	233984	233986 E	193474	193468 N
Stoke -		80 25 43 NE	233988		193462	
Stoke - -	Great Horks-	22 8 14 SW	195997	195997 E	175309	175309 N
Great Tey -	ley Steeple	51 49 14 NE	195997		175310	
Great Horksley	Mount Bures	87 34 46 NW	174196	174195 E	176230	176230 N
Stoke -	Steeple	66 32 14 SW	174194		176231	
Rushmere -	Hollesley	82 8 17 SE	322258	322258 E	210952	210953 N
Dover Court	Steeple	42 38 33 NE	322258		210955	
Rushmere -	Shottrisham	82 14 7 SE	311711	311720 E	212477	212477 N
Dover Court	Steeple	32 57 53 NE	311729		212477	
Woodbridge	Felixstow Sig-	16 45 47 SE	309444	309444 E	181100	181102 N
Dover Court -	nal Staff	64 33 24 NE	309444		181104	

* Finton, by mistake, in the former part of this Survey.

Names of stations.		Bearings.				Distance from meridian.	Mean.		Distance from perpendicular.	Mean.	
		°	′	″		Feet.	Feet.		Feet.	Feet.	
Woodbridge -	Bawdsey Sig-	33	7	47	SE	318758	318757	E	191712	191714	N
Dover Court	nal Staff	56	58	8	NE	318757			191716		
Butley -	Rendlesham	59	42	17	NW	313767	313761	E	239061	239058	N
Woodbridge	Steeple	57	12	43	NE	313755			239055		
Dover Court	Kesgrave	6	7	41	NW	278145	278145	E	216905	216904	N
Rushmere -	Steeple	81	3	47	SE	278140			216903		
Dover Court -	Waldringfield	20	4	29	NE	298893	298901	E	211282	211281	N
Rushmere -	Steeple	76	25	37	SE	298900			211282		
Dover Court	Wherstead	37	5	51	NW	258996	258995	E	200845	200844	N
Kesgrave -	Steeple	50	0	49	SW	258995			200843		
Capel Steeple	Hintlesham	3	26	17	NE	235016	235017	E	210623	210620	N
Stoke -	Steeple	56	11	25	NE	235019			210618		
Stoke - -	Bildestone	1	24	50	NE	202169	202168	E	230147	230147	N
Lavenham -	Steeple	89	50	0	SE	202168			230147		
Stoke - -	Aldham	35	18	30	NE	219654	219654	E	214079	214080	N
Bildeston -	Steeple	47	25	20	SE	219654			214081		
Naughton -	Hadleigh	1	12	40	SE	214492	214565	E	207881	207823	N
Lavenham -	Steeple	58	15	30	SE	214639			207766		
Naughton -	Lindsey	49	42	50	SW	199105	199177	E	216365	216308	N
Lavenham -	Steeple	56	15	10	SE	199249			216251		
Stoke -	Newton	51	25	50	NW	179897	179897	E	204850	204850	N
Lavenham -	Steeple	3	29	40	SE	179897			204850		
Stoke -	Grotton	24	24	50	NW	193001	193001	E	205823	205823	N
Newton -	Steeple	85	45	10	NE	193001			205823		
Bulmer -	Waldingfield	62	39	10	NE	177687	177690	E	213569	213540	N
Glemsford -	Steeple	58	26	20	SE	177693			213511		
Glemsford -	Acton Steeple	59	48	35	SE	171295	171295	E	218047	218047	N
Lavenham -		30	5	35	SW	171296			218047		
Bulmer -	Beauchamp	51	50	10	NW	140480	140478	E	213137	213135	N
Lavenham -	Ch. St. Paul's	25	43	20	SW	140476			213134		
Lavenham -	Hedingham	45	20	20	SW	136127	136128	E	188492	188492	
Toppesfield	Castle	70	0	30	SE	136129			188492		
Lavenham -	Ridgewell	66	27	10	SW	121213	121213	E	205317	205316	
Bulmer -	Steeple	34	1	50	NW	121214			205316		
Naughton -	Langham	2	52	7	SE	216543	216610	E	179186	179127	
Stoke -	Steeple	10	22	7	SE	216690			179069		
Stoke -	Earles Colne	30	43	14	SW	159472	159471	E	164565	164565	
St. Mary's, Colch.	Steeple	72	35	26	NW	159471			164565		
St. Mary's -	West Bergholt	51	8	42	NW	189552	189553	E	161393	161393	
Great Tey -	Steeple	70	49	58	NE	189554			161393		
Danbury -	Braxted	76	18	45	NE	155021	155021	E	120989	120988	
Great Tey -	Steeple	23	16	5	SW	155022			120988		
Braxted -	Kelverdon	11	32	29	NE	157104	157104	E	131184	131185	
Great Tey -	Steeple	27	53	29	SW	157104			131186		
Great Tey -	Messing	2	21	21	SE	170301	170301	E	132010	132009	
Kelverdon -	Steeple	86	25	29	NE	170302			132009		
Great Tey -	East Thorp	25	49	41	SE	175627	175627	E	140237	140236	
Kelverdon -		63	57	29	NE	175627			[illegible]		

Names of stations.		Bearings.	Distance from meridian.	Mean.		Distance from perpendicular.	Mean.	
		° ′ ″	Feet.	Feet.		Feet.	Feet.	
Tiptree -	Witham Steep.	56 46 32 NW	143890	143888	E	120923	120927	N
Danbury -		21 52 28 NE	143887			120932		
Tiptree -	Tarling Steeple	75 58 23 NW	129390	129388	E	119511	119513	N
Danbury -		2 3 23 NW	129386			119516		
Pleshey - -	Felstead Steeple	13 41 16 NE	98214	98210	E	138616	138613	N
Danbury -		32 23 31 NW	98207			138611		
Pleshey -	Great Leigh Steeple	81 44 16 NE	118041	118041	E	122359	122361	N
Felstead -		50 39 44 SE	118041			122363		
Pleshey -	Great Baddow Steeple	33 23 53 SE	114190	114191	E	87232	87229	N
Danbury -		88 27 53 SW	114191			87226		
Pleshey -	Chelmsford Steeple	29 41 53 SE	107349	107349	E	94258	94263	N
Danbury -		74 2 1 NW	107350			94288		
Danbury -	Whittle Steeple	82 41 29 NW	97251	97250	E	91931	91936	N
Pleshey -		8 41 33 SE	97249			91942		
Gallywood -	Roxwell Steeple	43 43 26 NW	86985	86986	E	99902	99903	N
Pleshey -		18 43 10 SW	86987			99905		
Gallywood -	White Roding Steeple	51 38 40 NW	59967	59967	E	116602	116604	N
Pleshey -		86 15 55 SW	59967			116606		
Frierning -	Doddinghurst Steeple	78 55 28 SW	67638	67644	E	69301	69304	N
Southweald		20 48 47 NE	67651			69308		
Southweald -	Theydon Mount Steep.	54 3 14 NW	36122	36125	E	70851	70859	N
Epping Mill		65 23 14 SE	36128			70867		
Southweald -	Navestock new Mill	4 50 14 NW	60484	60485	E	62217	62221	N
Theydon Mount		70 29 14 SE	60487			62226		
Southweald	Theydon Garnon Steeple	59 21 14 NW	29374	29363	E	71511	71461	N
Theydon Mount		85 20 14 NW	29351			71411		
Theydon Mount	Havering Steeple	16 39 44 SE	42172	42177	E	50654	50654	N
Theydon Garnon		31 42 14 SE	42183			50654		
Severndroog	Cupola at Woodford	10 24 56 NW	4584	4584	E	47329	47331	N
Highbeech -		10 4 28 SW	4584			47333		
Southweald	Ruins near Ilford	69 1 52 SW	13358	13357	E	34223	34225	N
Highbeech -		9 1 28 SE	13356			34227		
Nasing -	Hunsdon Steeple	5 57 9 NE	12836	12839	E	115758	115758	N
Berkhampstead Gazebo -		71 6 45 NE	12843			115758		
Huntsdon -	Broxbourn Steeple	40 38 9 SW	3021	3020	W	97376	97377	N
Nasing -		88 37 51 NW	3020			97378		
Danbury -	Willingale Spain Steeple	81 23 51 NW	70724	70725	E	96710	96713	N
Hatfield Oak		26 38 16 SE	70726			96716		
Danbury -	Braintree Steeple	5 52 1 NW	124508	124510	E	146272	146266	N
Felstead -		73 47 14 NE	124513			146261		
Hatfield Oak	Harlow Steep.	52 44 34 SW	33842	33846	E	111140	111081	N
Berkhampstead Gazebo -		81 29 13 NE	33850			111023		
Hatfield Oak	Sabridgeworth Steeple	74 50 17 SW	34982	34959	E	121958	122044	N
Nasing -		43 48 2 NE	34936			122136		

Names of stations.		Bearings. °	′	″		Distance from meridian. Feet.	Mean. Feet.	Distance from perpendicular. Feet.	Mean. Feet.
Albury -	Bishop Stortford Steeple	54	29	24	SE	35934	35929 E	143459	143454 N
Henham -		40	22	56	SW	35925		143450	
Stanstead Mountfitchet Steeple	Farnham Steeple	79	27	59	NW	34427	34425 E	154725	154723 N
Henham -		69	37	6	SW	34423		154721	
Albury -	Stanstead Mountfitchet Steeple	84	18	27	SE	47531	47526 E	152294	152289 N
Henham -		29	34	6	SW	47522		152285	
Henham -	Meesdon Mill	69	20	30	NW	19158	19162 E	180479	180483 N
Albury -		1	6	40	NW	19167		180488	
Rickling -	Newport Steeple	41	50	8	NE	48409	48408 E	185013	185010 N
Elmdon -		45	1	52	SE	48407		185007	
Balsham -	ShudyCamps*	25	17	50	SE	82115	82114 E	217814	217818 N
Elmdon -		74	57	40	NE	82114		217822	
Shudy Camps	Ashdon Steeple	55	23	10	SW	68848	68848 E	208661	208660 N
Balsham -		5	49	20	SW	68848		208659	

Bearings of the principal Stations of the western Parts of Kent, &c. &c

Frant -	Sevenoaks Mill	19	20	53	NW	42962	42961 E	83270	83270
Botley Hill -		76	21	7	SE	42961		83271	
Frant - -	Chiddingstone Steeple	41	38	3	NW	33852	33850 E	106413	106403
Sevenoaks Mill		21	31	57	SW	33848		106394	
Frant -	Mount Sion	6	35	46	NW	55693	55691 E	80967	80962
Chiddingstone		40	38	14	NE	55690		80957	
Frant -	East Peckham Steeple	24	52	44	NE	87147	87146 E	84966	84963
Mount Sion		82	45	16	SE	87145		84960	
East Peckham	Tudeley Steeple	32	3	44	SW	73465	73465 E	106804	106803
Mount Sion		34	31	16	SE	73465		106803	
Sevenoaks -	Seal Chart	65	21	5	NE	59674	59673 E	75602	75601
Botley Hill		87	22	55	SE	59673		75601	
Sevenoaks	Tunbridge Steeple	47	49	55	SE	63789	63789 E	102135	102135
Seal Chart -		8	48	55	SE	63789		102135	
Seal Chart	Otford Mount	36	37	55	NW	47503	47503 E	59233	59233
Sevenoaks -		10	42	5	NE	47503		59234	
Norwood -	Station, Well Hill	68	30	7	SE	34094	34093 E	45635	45636
Severndroog		25	45	47	SE	34093		45638	
Well Hill -	Crayford Steeple	9	14	45	NE	40174	40173 E	8287	8287
Severndroog		80	50	1	SE	40173		8288	
Crayford -	Ash Steeple	38	53	55	SE	68780	68780 E	43740	43741
Well Hill -		86	52	25	NE	68780		43742	
Gad's Hill -	Northfleet Steeple	69	4	54	NW	76615	76614 E	12548	12548
Halstow -		84	36	53	SW	76614		12549	
Sheppey -	Hern Hill	48	45	30	SE	220216	220216 E	60633	60633
Prinstead -		80	31	12	NE	220217		60633	
Hern Hill -	Stockbury Steeple	85	47	30	NW	148806	148806 E	55379	55379
Sheppey -		39	3[illegible]	30	SW	148806		55379	
	[illegible]	15	2[illegible]	54	SW	—	163132 E	—	69824[illegible]

Bearings of the secondary and inferior Objects, &c. of the western Parts of Kent.

Names of stations.		Bearings.	Distance from meridian.	Mean.	Distance from perpendicular	Mean.
		° ′ ″	Feet.	Feet.	Feet.	Feet.
Frant - -	Bidborough Steeple	16 51 0 NW	54785	54786 E	113503	113506 S
Botley -		53 21 9 SE	54787		113496	
Frant -	Station near Bidborough Church	20 46 3 NW	52687	52685 E	113000	112995 S
Chiddingstone		70 43 3 SE	52684		112991	
Botley - -	Tree near Kibben's Cross	57 9 11 SE	83513	83515 E	126687	126689 S
Frant -		60 56 36 NE	83518		126691	
Frant - -	Cowden Steeple	67 18 3 NW	23647	23679 E	122273	122270 S
Station near Bidborough Church		72 17 27 SW	23712		122268	
Station near Bidborough -	Leigh Steeple	15 18 3 NW	49728	49731 E	102152	102164 S
Mount Sion		15 39 57 SW	49742		102175	
Frant - -	Station, Ide Hill	31 32 33 NW	29621	29616 E	85152	85142 S
Chiddingstone		11 16 33 NW	29611		85132	
Ide Hill -	Eatonbridge* Steeple	38 26 27 SW	15562	15569 E	102848	102842 S
Chiddingstone		78 58 33 NW	15557		102839	
Mount Sion	Hadlow Steeple	62 9 16 SE	78053	78055 E	92775	92777 S
Peckham -		49 18 44 SW	78058		92779	
Otford Mount	Sundrich Steeple	49 33 5 SW	29901	29901 E	74241	74241 S
Seal Chart -		87 22 55 NW	29901		74241	
Well Hill -	Ketson Common Windmill	86 10 47 NW	7615	7614 E	43868	43867 S
Norwood -		54 24 45 SE	7614		43867	
Well Hill -	Hayes Common Flagstaff	82 24 47 NW	6068	6068 E	41903	41904 S
Severndroog		11 53 13 SW	6068		41905	
Hayes Common	Addington Common Flagstaff	84 34 43 SW	11919	11912 E	43611	43608 S
Norwood -		21 16 47 SE	11906		43606	
Well Hill -	Station, Farnborough	84 46 47 NW	22492	22491 E	44576	44576 S
Severndroog		11 47 47 SE	22491		44577	
Farnborough	St. Mary's Cray	15 41 13 NE	26553	26553 E	30116	30116 S
Well Hill -		25 54 47 NW	26554		30116	
Well Hill -	Halstead Steeple	31 47 27 SW	29536	29535 E	52990	52990 S
Norwood -		59 50 3 SE	29535		52990	
Norwood -	Bromley Steeple	85 1 7 SE	3304	3303 E	26574	26573 S
Severndroog		25 29 23 SW	3303		26572	
Bromley -	Hayes Steeple	6 39 47 SE	4441	4447 E	36311	36318 S
Well Hill -		72 33 42 NW	4453		36326	
Bromley -	Lewisham Steeple	19 48 [illegible] NW	3352	3353 W	8096	8096 S
Severndroog		76 57 23 SW	3354		8097	
Chiselhurst -	New Cross	51 47 59 NW	9490	9490 W	3550	3550 S
Severndroog		88 43 59 NW	9491		3550	

* Edenbridge, by mistake, in the former part of this Survey.

Names of stations.		Bearings.				Distance from meridian.	Mean.		Distance from perpendicular.	Mean.	
		°	′	″		Feet.	Feet.		Feet.	Feet.	
Severndroog	Eastcombe Point	50	43	59	NW	86	86	E	7332	7332	S
New Cross -		41	21	1	NE	87			7332		
Severndroog	Woolwich Steeple	1	4	59	NW	13855	13853	E	5556	5556	N
Eastcombe Point		82	38	59	SE	13852			5557		
Crayford -	Bexley Steeple	41	21	39	SW	35426	35425	E	13679	13680	S
Severndroog		65	48	31	SE	35425			13681		
Well Hill -	Charlton Farm	52	33	15	NW	15965	15965	E	31752	31751	S
Crayford -		45	53	45	SW	15965			31751		
Crayford -	Dartford Brent Mill	69	26	13	SE	54059	54060	E	13496	13496	S
Ash - -		25	57	6	NW	54061			13497		
Ash - -	Hartley Steeple	29	56	5	NE	72706	72711	E	36922	36920	S
Northfleet -		9	16	55	SW	72716			36918		
Ash - -	Ridley Steeple	64	4	45	SE	73446	73446	E	46009	46009	S
Northfleet -		5	32	35	SW	73446			46010		
Gads Hill -	Cliff Steeple	24	3	15	NE	113741	113741	E	5396	5396	S
Gravesend -		70	49	7	NE	113742			5397		
Halstow -	Gravesend Steeple	84	19	23	SW	84513	84517	E	12033	12058	S
Gravesend -		6	19	23	NW	84518			12082		
Halstow -	Chalk Steeple	71	45	9	SW	96111	96110	E	18464	18462	S
Gravesend -		74	54	24	SE	96109			18460		
Gads Hill -	Guard Room, Lower Hope Point	3	41	11	NE	107247	107247	E	1895	1894	N
Gravesend -		52	13	26	NE	107247			1894		
Gads Hill -	Flagstaff, Tilbury Fort	52	58	28	NW	86260	86260	E	9039	9040	S
Gravesend -		12	6	7	NE	86260			9041		
Sheppey -	Rainham Steeple	62	21	30	SW	139485	139484	E	41373	41372	S
Gads Hill -		62	21	33	SE	139483			41372		
Halstow -	Swanscombe Spire	84	51	25	SW	70463	70463	E	12880	12879	S
Gads Hill -		72	59	24	NW	70464			12879		
Halstow -	Southfleet Steeple	75	19	30	SW	73187	73187	E	22234	22234	S
Gravesend -		59	50	13	SW	73187			22234		
Gravesend -	Shorn Mill	61	26	17	SE	98017	98025	E	22580	22584	S
Halstow -		64	12	53	SW	98034			22586		
Sheppey -	Gillingham Steeple	78	54	49	SW	128724	128724	E	31425	31426	S
Halstow -		0	45	32	SW	128725			31427		
Gillingham -	St. James's, Isle of Grain	54	20	32	NE	163871	163871	E	6210	6209	S
Sheppey -		37	57	19	NW	163871			6209		
Sheppey -	Friendsbury Steeple	87	3	26	NW	115632	115631	E	18991	18991	S
Gads Hill -		65	10	37	NE	115631			18991		
Sheppey -	Star Inn	71	18	17	SW	128656	128656	E	38221	38221	S
Halstow -		0	43	10	SW	128656			38222		
Sheppey -	Upper Bell Inn	62	18	51	SW	116532	116532	E	53454	53441	S
Halstow -		15	16	0	SW	116533			53431		
Sheppey -	Upchurch Steeple	63	9	35	SW	148260	148250	E	36283	36266	S
Gads Hill -		73	30	35	SE	148240			36251		
Frinsted -	Hucking Spire	77	54	35	NW	147823	147822	E	66545	66545	[illegible]
Sheppey -		[illegible]	[illegible]	[illegible]	SW	[illegible]			[illegible]		

Names of stations.		Bearings. ° ′ ″	Distance from meridian. Feet.	Mean. Feet.	Distance from perpendicular. Feet.	Mean. Feet.
Hern Hill -	East Church	34 30 32 NW	196669	196678 E	26383	26376 S
Sheppey -		78 12 36 SE	196688		26369	
East Church -	Milton Steeple	57 27 7 SW	169440	169445 E	43761	43758 S
Sheppey -		17 29 46 SW	169450		43754	
Milton -	Iwade Steeple	14 54 14 NW	165705	165702 E	29703	29685 S
Sheppey -		54 26 16 SW	165700		29667	
Hern Hill -	Witchling	73 23 12 SW	170790	170789 E	75380	75380 S
Frinstead -	Steeple	54 2 13 SE	170789		75380	
Hern Hill -	Sheldwich	56 6 30 SW	202302	202291 E	72667	72675 S
Sheppey -	Steeple	27 12 48 SE	202280		72682	
Sheldwich -	Queenborough	31 53 48 NW	170208	170213 E	21124	21128 S
Sheppey -	Steeple	80 52 4 NW	170219		21133	
Halstow -	St. Mary's	77 55 24 NE	135978	135977 E	6123	6123 S
Hadleigh -	Steeple	4 8 17 SE	135977		6123	
Hern Hill -	Feversham	77 56 30 NW	204931	204930 E	57368	57368 S
Sheppey -	Steeple	39 6 8 SE	204930		57368	

Latitudes and Longitudes of the preceding Stations and Objects, referred to the Meridian of Greenwich.

Names of stations.	Latitudes. ° ′ ″	Longitudes. ° ′ ″
Highbeech -	51 39 42,5	0 2 8,3 E
Station, Hampstead -	51 33 55,4	0 10 28,0 W
New Station, Wrotham -	51 18 55,5	0 18 49,2 E
Station, Gravesend - -	51 26 5,9	0 22 15,6 E
Langdon Hill	51 33 12,5	0 25 22,1 E
Hadleigh -	51 32 52,5	0 35 7,4 E
Halstow - -	51 27 20,3	0 33 50,7 E
Gads Hill -	51 24 43,8	0 27 40,2 E
Sheppey -	51 24 23,2	0 46 11,5 E
Rayleigh -	51 35 17,0	0 36 29,2 E
Prittlewell -	51 32 56,2	0 42 16,2 E
Canewdon -	51 37 3,4	0 46 15,5 E
Staff, Sheerness	51 11 21,6	0 44 25,7 E
Danbury -	51 42 59,3	0 34 26,0 E
Frierning -	51 40 32,5	0 22 7,8 E
Purfleet Cliff -	51 28 59,4	0 14 9,9 E
South End -	51 33 4,4	0 42 15,5 E
Staff, Shoeburyness -	51 31 19,1	0 47 12,6 E

Names of stations.	Latitudes. ° ′ ″	Longitudes. ° ′ ″
Tiptree - -	51 47 2,2	0 41 17,8 E
Tillingham -	51 41 52,7	0 52 52,9 E
Peldon -	51 58 50,3	0 53 11,4 E
Flagstaff, St. Osyth Priory -	51 47 57,9	1 4 25,7 E
Great Tey -	51 53 53,2	0 44 49,9 E
Stoke -	51 59 20,4	0 53 22,6 E
Thorp -	51 51 23,2	1 9 38,3 E
Little Bentley	51 52 56,3	1 4 49,7 E
Dover Court	51 55 59,1	1 15 6,1 E
St. Mary's, Colchester -	51 53 17,7	0 53 33,7 E
West Mersea	51 46 29,8	0 54 33,3 E
Little Bromley	51 54 43,4	0 39 16,8 E
Tattingstone	51 59 39,4	0 41 55,5 E
Rushmere -	52 4 7,3	1 12 0,8 E
Falkenham -	51 56 2,2	1 20 4,0 E
Woodbridge -	52 5 34,6	1 18 36,8 E
Butley -	52 5 53,7	1 27 39,8 E
Orford Light House -	52 5 0,1	1 34 13,6 E
Otley -	52 8 54,1	1 13 2,5 E

Names of stations.	Latitudes. °	′	″	Longitudes. °	′	″	
Henley - -	52	7	2,9	1	8	57,2	E
Copdock -	52	1	51,9	1	5	13,2	E
Naughton -	52	6	3,5	0	56	56,7	E
Twinstead -	51	59	48,4	0	42	47,2	E
Lavenham -	52	6	19,1	0	47	27,0	E
Bulmer - -	52	1	41,5	0	41	8,6	E
Glemsford -	52	6	8,8	0	40	36,4	E
Toppesfield -	52	0	28,1	0	32	4,1	E
Gallywood Common -	51	41	51,8	0	27	47,7	E
Pleshey -	51	48	8,0	0	24	40,8	E
High Easter -	51	48	26,9	0	20	56,1	E
Hatfield Oak	51	49	35,5	0	14	37,4	E
Beauchamp Roding - -	51	45	48,9	0	17	8,8	E
Thaxted -	51	57	13,1	0	20	32,7	E
Southweald -	51	37	17,4	0	16	8,1	E
Brentwood -	51	37	11,8	0	18	9,5	E
New Station, Highbeech -	51	39	42,9	0	2	1,1	E
Epping Mill	51	41	23,3	0	5	43,8	E
Berkhampstead Gazebo -	51	45	23,0	0	7	23,3	E
Henham on the Mount -	51	56	1,7	0	14	45,7	E
Thorley -	51	50	53,8	0	8	33,2	E
Elmdon - -	52	2	7,3	0	8	3,8	E
Rickling -	51	57	40,3	0	10	51,2	E
Albury - -	51	54	8,1	0	5	12,4	E
Balsham -	52	7	56,1	0	19	9,6	E
Babraham Mount	51	32	38,5	0	12	52,1	E
Triplow -	52	6	5,0	0	6	21,3	E
Hornchurch -	51	33	37,3	0	13	36,1	E
Barking -	51	32	7,5	0	4	36,5	E
Westham -	51	32	10,6	0	0	35,7	E
Chigwell - -	51	37	27,2	0	4	53,4	E
Billericay - -	51	37	32,5	0	25	6,5	E
Public House	51	30	56,3	0	5	59,6	E
Rainham -	51	31	5,7	0	11	29,0	E
Belvidere -	51	29	11,7	0	9	55,3	E
Valence Tree	51	34	39,6	0	8	14,2	E
Cold Harbour	51	29	16,5	0	11	19,3	E
Chadwell -	51	28	53,4	0	22	10,9	E
West Tilbury	51	28	26,1	0	23	27,7	E
Grays Steeple	51	29	1,7	0	18	30	E
West Thurrock	51	28	20,0	0	17	34,2	E
Northfleet -	51	26	34,6	0	20	5,4	E
Horndon -	51	31	25,7	0	24	17,8	E

Names of stations.	Latitudes. °	′	″	Longitudes. °	′	″	
Flagstaff, East Tilbury -	51	27	36,0	0	25	49,2	E
Fobbing Steeple	51	31	39,8	0	28	30,7	E
Thundersley -	51	34	7,4	0	34	13,9	E
Leigh - -	51	32	28,7	0	39	12,6	E
Little Wakering	51	33	38,0	0	47	18,3	E
Bank Flagstaff	51	33	26,0	0	50	37,5	E
Foulness Chapel	51	36	5,7	0	53	28,1	E
Tillingham Grange Signal Staff - -	51	40	6,2	0	55	15,2	
Flagstaff, Bradwell Point -	51	44	5,0	0	56	19,8	E
Brightlingsea	51	49	42,3	1	0	39,5	E
Toleshunt Major	51	45	57,2	0	45	49,5	E
Tolesbury -	51	45	27,6	0	49	54,9	E
Althorn -	51	39	23,8	0	45	26,8	E
Burnham -	51	38	17,7	0	48	48,2	E
Rettenden -	51	38	5,2	0	33	22,4	E
Runwell -	51	37	15,6	0	31	53,4	E
Great Burstead	51	36	13,6	0	25	31,2	E
East Hanningfield	51	40	11,4	0	33	10,2	E
Hockley -	51	36	34,9	0	38	6,3	E
Stow, St. Mary's	51	39	47,4	0	38	57,1	E
Stock Steeple -	51	39	40,0	0	26	19,3	E
Southminster	51	39	42,7	0	49	44,0	E
Layer Marney	51	49	13,7	0	47	42,6	E
St. Osyth Point Signal Staff -	51	47	3,0	1	8	46,5	E
Great Clackton Signal Staff	51	48	12,1	1	12	5,9	E
Frinton Steeple	51	30	26,8	1	12	28,4	E
Flagstaff, Frinton	51	50	17,8	1	15	33,4	E
Walton Tower	51	51	51,2	1	17	6,8	E
Cupola, Languard Fort -	51	56	18,5	1	19	3,9	E
Ardleigh -	51	55	34,3	1	59	1,5	E
Frating -	51	51	38,2	1	1	12,8	E
Thorrington -	51	51	10,0	1	3	19,4	E
Kirby - -	51	51	9,3	1	13	7,4	E
Brantham -	51	57	56,4	1	4	15,5	E
Harwich -	51	56	43,3	1	17	7,8	E
Little Oakley	51	54	37,3	1	12	41,9	E
Bawdsey -	52	0	38,8	1	24	52,1	E
[illegible]stead -	51	58	20,0	1	11	25,2	E
[illegible]warton -	51	57	[illegible]	1	13	42,9	E
Bradfield -	51	56	[illegible]	1	6	56,5	E
Orford -	52	[illegible]	40,9	1	31	54,2	E

Names of stations.	Latitudes.	Longitudes.
	° ′ ″	° ′ ″
Nacton -	52 0 34,5	1 13 34,6 E
Capel -	52 0 10,6	1 2 6,9 E
Great Horksley	51 57 16,5	0 51 58,4 E
Mount Bures -	51 57 27,8	0 46 11,7 E
Hollesley -	52 2 48,7	1 25 38,4 E
Shottisham -	52 3 5,2	1 22 50,8 E
Felixstow Staff	51 57 56,9	1 22 5,1 E
Bawdsey Signal Staff -	51 59 39,8	1 24 36,6 E
Rendlesham -	52 7 27,2	1 23 31,5 E
Kesgrave -	52 7 14,9	1 14 2,2 E
Waldringfield -	51 56 56,1	1 19 26,0 E
Whertstead -	52 1 19,6	1 8 47,1 E
Hintlesham -	52 2 59,4	1 2 27,3 E
Bildestone -	52 1 50,5	0 53 40,0 E
Aldham -	52 3 35,5	0 58 23,1 E
Hadleigh -	52 2 34,5	0 57 0,7 E
Lindsey -	52 5 40,5	0 52 58,9 E
Newton -	52 2 9,1	0 47 47,4 E
Grotton -	52 2 23,6	0 32 21,1 E
Waldingfield -	52 3 35,1	0 47 13,7 E
Acton -	52 3 31,2	0 45 31,7 E
Beauchamp -	52 3 34,4	0 37 20,2 E
Hedingham Castle -	51 59 35,6	0 36 7,6 E
Ridgewell -	52 2 18,8	0 32 12,1 E
Langham -	51 57 51,6	0 57 28,1 E
Earles Colne	51 55 34,2	0 42 15,6 E
West Bergholt	51 55 0,1	0 50 13,3 E
Braxted -	51 48 25,5	0 40 58,4 E
Kelvedon -	51 50 5,5	0 41 33,0 E
Messing -	51 50 12,5	0 45 2,5 E
East Thorp -	51 51 33,2	0 46 28,4 E
Witham -	51 53 34,4	0 38 6,1 E
Tarling -	51 48 13,0	0 34 11,7 E
Willingale Spain	51 44 31,6	0 18 40,0 E
Braintree -	51 52 33,7	0 32 57,5 E
Felstead -	51 51 23,3	0 25 59,2 E
Great Leigh -	51 48 41,8	0 31 7,8 E
Great Baddow	51 42 55,8	0 30 7,2 E
Chelmsford -	51 44 5,8	0 28 19,7 E
Whittle -	51 43 43,4	0 25 39,6 E
Roxwell -	51 45 2,3	0 22 57,7 E
White Roding	51 47 48,2	0 15 50,8 E
Doddinghurst	51 40 1,8	0 17 49,4 E
Theydon Mount	51 40 18,0	0 9 31,1 E
Navestock Mill	51 38 52,2	0 15 55,8 E
Theydon Garnon	51 40 23,6	0 7 44,2 E
Havering -	51 36 58,7	0 11 6,1 E

Names of stations.	Latitudes.	Longitudes.
	° ′ ″	° ′ ″
Cupola at Wood ford -	51 36 26,5	0 1 12,3 E
Ruins near Ilford	51 34 17,3	0 3 30,7 E
Hunsdon -	51 47 40,8	0 3 23,5 E
Broxbourn -	51 44 30,8	0 0 47,8 E
Harlow -	51 46 54,4	0 5 38,4 E
Sabridgeworth	51 48 42,5	0 9 14,4 E
Bishop Stortford	51 52 13,4	0 9 30,5 E
Stanstead Mountfitchet -	51 53 40,2	0 12 35,1 E
Farnham -	51 54 4,4	0 9 7,0 E
Windmill, Meesdon -	51 58 18,5	0 5 4,9 E
Newport -	31 59 2,5	0 12 50,6 E
Shudy Camps	52 4 24,2	0 21 49,9 E
Ashdon -	52 2 54,7	0 18 17,6 E

West Parts of Kent.

Windmill, Sevenoaks -	51 14 58,5	0 11 12,9 E
Chiddingstone	51 11 10,6	0 8 49,6 E
Station, Mount Sion -	51 15 20,8	0 14 32,6 E
East Peckham	51 14 40,1	0 22 45,2 E
Tudeley - -	51 11 6,0	0 19 9,4 E
Seal Chart -	51 16 13,6	0 15 35,3 E
Tunbridge -	51 11 51,6	0 17 1,6 E
Otford Mount	51 18 55,3	0 12 25,3 E
Well Hill -	51 21 9,8	0 8 55,3 E
Crayford -	51 27 17,8	0 10 32,2 E
Ash - -	51 21 26,9	0 18 0,2 E
Bidborough -	51 10 0,3	0 14 6,8 E
Station near Bidborough Church	51 10 4,0	0 13 44,3 E
Tree near Kibben's Cross	51 7 48,8	0 21 45,1 E
Cowden Steeple	51 7 34,2	0 6 9,9 E
Leigh Steeple -	51 11 51,8	0 12 58,3 E
Ide Hill -	51 14 40,3	0 7 43,9 E
Eatonbridge -	51 10 6,0	0 4 3,3 E
Hadlow -	51 13 23,4	0 20 22,3 E
Sundrich -	51 16 27,7	0 8 8,7 E
Windmill, Ketson Common -	51 22 27,5	0 1 59,6 E
Hayes Common Flagstaff -	51 21 46,9	0 1 35,3 E

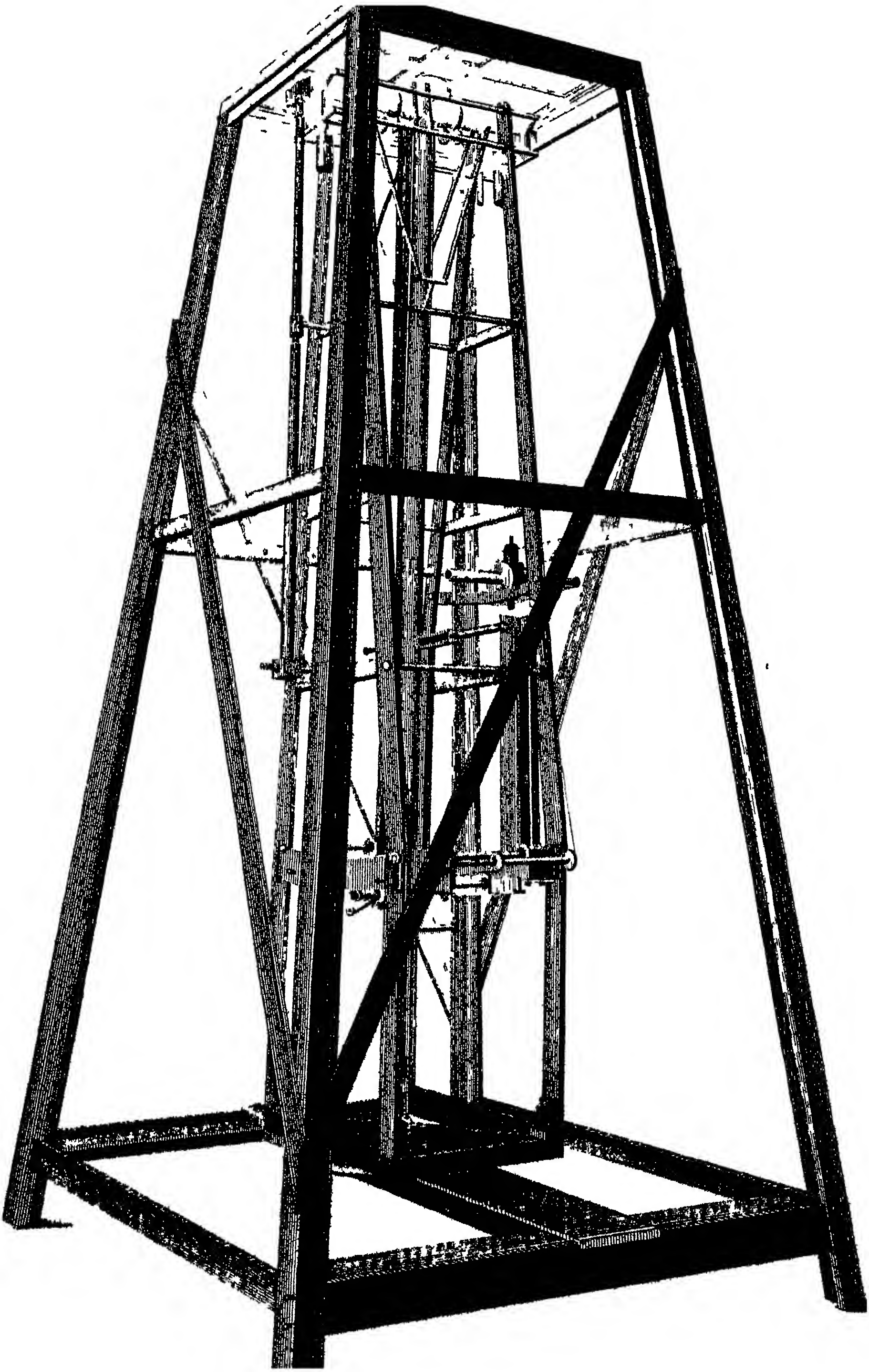
Names of stations.	Latitudes	Longitudes.
	° ′ ″	° ′ ″
Addington Common Flagstaff	51 21 30,1	0 3 6,6 E
Farnborough	51 21 20,4	0 5 53,2 E
St. Mary's Cray	51 23 42,9	0 6 57,3 E
Halstead -	51 19 57,3	0 7 43,6 E
Bromley -	51 24 17,8	0 0 51,9 E
Hayes - -	51 22 41,3	0 1 9,8 E
Lewisham -	51 27 20,2	0 0 52,7 W
Station, New Cross	51 28 5,1	0 2 29,3 W
Eastcombe Point	51 29 52,2	0 0 1,3 E
Woolwich -	51 29 34,6	0 3 38,2 E
Bexley -	51 26 24,8	0 9 17,3 E
Charlton Farm	51 23 27,0	0 4 10,9 E
Dartford Brent Mill -	51 26 26,1	0 14 10,5 E
Hartley -	51 22 34,5	0 19 2,3 E
Ridley -	51 21 4,9	0 19 13,3 E
Cliff Steeple -	51 27 43,1	0 29 50,2 E
Gravesend Steeple	51 27 39,2	0 22 9,7 E
Chalk Steeple	51 25 35,4	0 25 11,5 E
Guard Room, Lower Hope Point -	51 28 55,3	0 28 8,6 E
Flagstaff, Tilbury Fort -	51 27 8,8	0 22 37,4 E

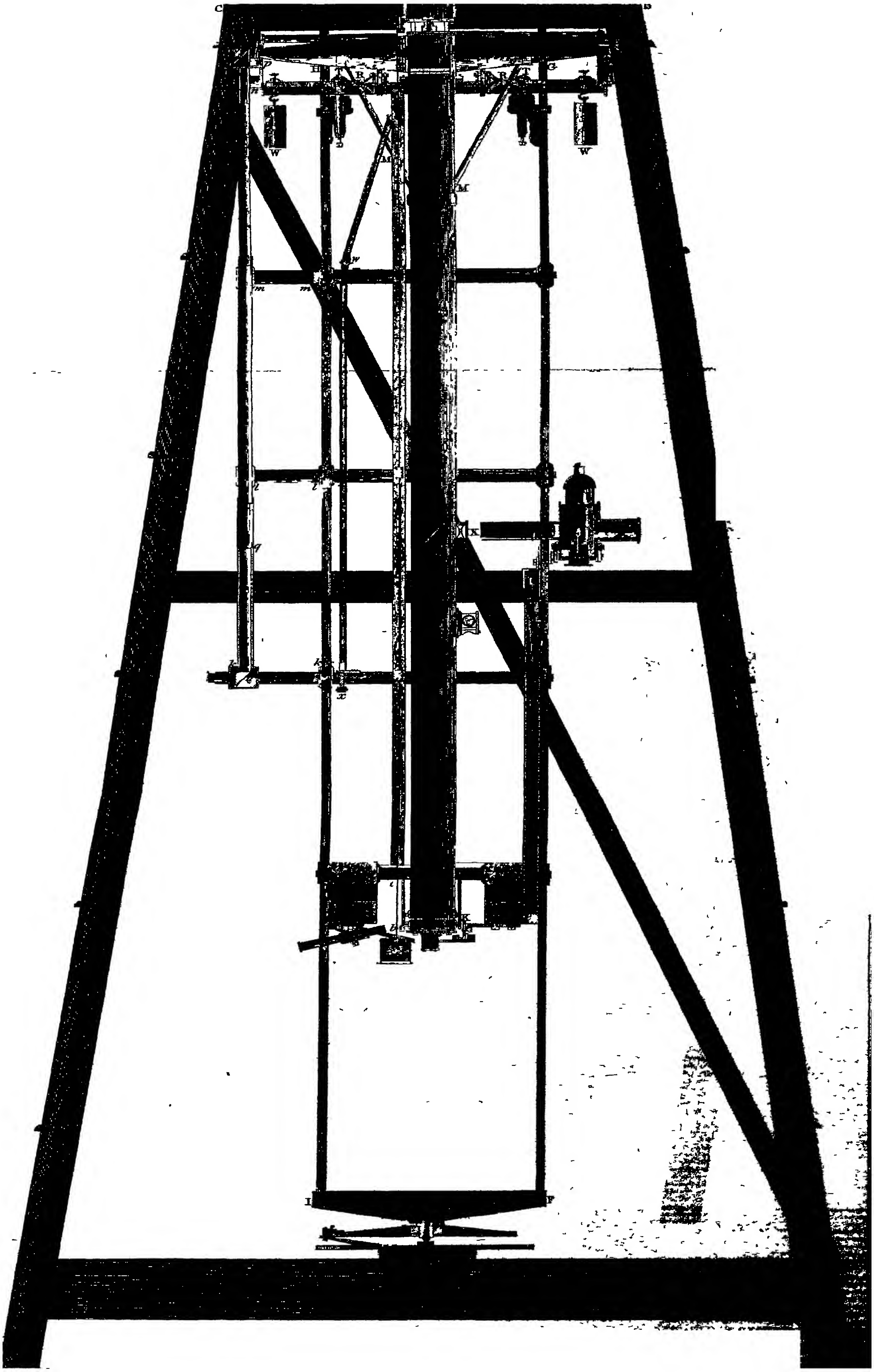
Names of stations.	Latitudes	Longitudes.
	° ′ ″	° ′ ″
Rainham -	51 21 46,5	0 36 30,7 E
Southfleet -	51 24 59,4	0 19 10,7 E
Shorn Mill -	51 24 54,7	0 25 41,3 E
Gillingham -	51 23 27,4	0 37 3,0 E
St. James's, Isle of Grain -	51 27 36,9	0 54 6,9 E
Friendsbury	51 25 29,0	0 30 18,4 E
Star Inn -	51 22 18,5	0 37 1,1 E
Upper Bell Inn	51 19 49,2	0 30 28,9 E
Upchurch -	51 22 36,1	0 38 49,2 E
Bobbing -	51 21 13,6	0 42 34,0 E
Frinstead -	51 17 3,9	0 42 37,9 E
Hern Hill -	51 18 28,2	0 57 34,9 E
Stockbury -	51 19 27,6	0 38 55,3 E
Hucking -	51 17 37,6	0 38 38,3 E
East Church -	51 24 8,8	0 51 31,9 E
Milton -	51 21 20,3	0 44 21,0 E
Iwade -	51 23 39,5	0 43 24,4 E
Witchling -	51 16 8,4	0 44 36,8 E
Sheldwich -	51 16 31,6	0 52 51,4 E
Queenborough	51 25 3,4	0 44 36,5 E
St. Mary's -	51 27 34,4	0 53 40,1 E
Feversham -	51 19 2,3	0 53 35,7 E

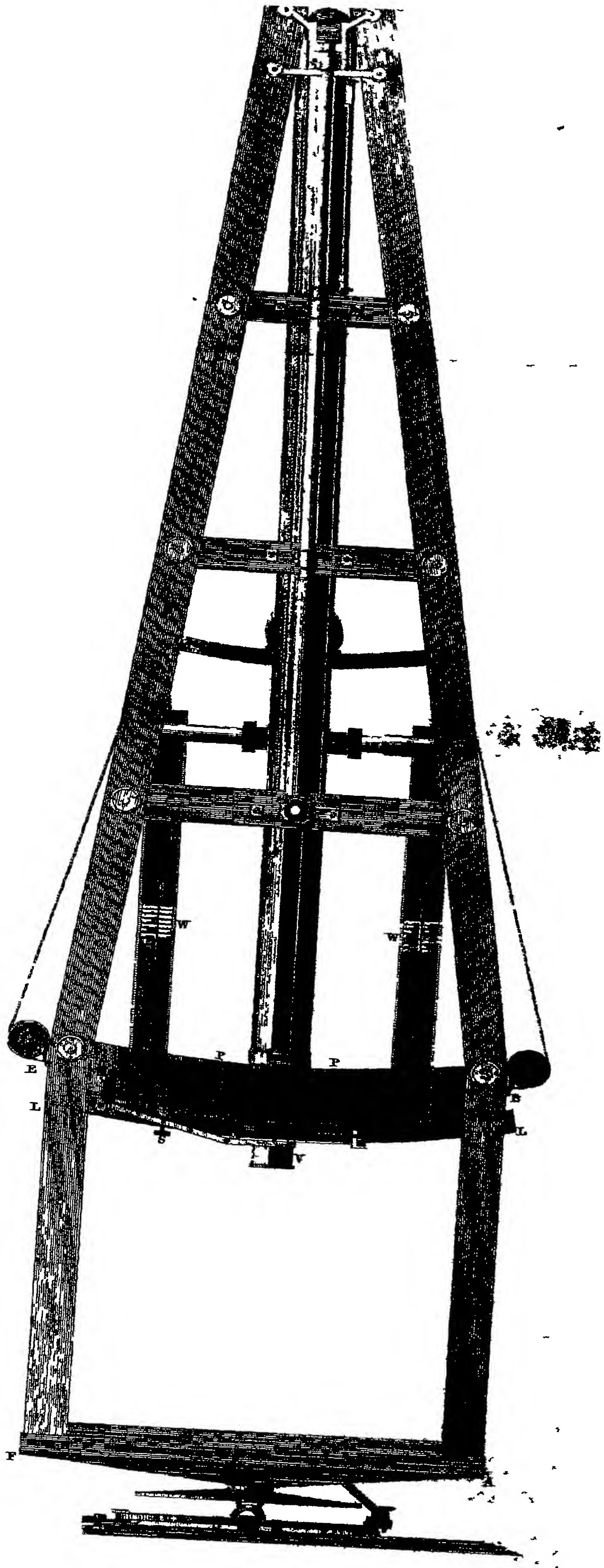
In page 399, line 13, *for* G, *read* g.

—— 415, lines 5 and 6 from the bottom, *for* 430 revolutions, *read* 436.

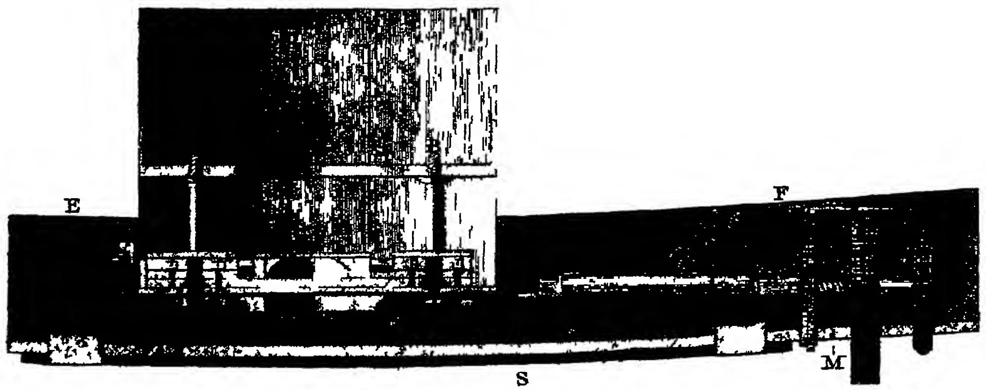
—— 468, line 3, *for* 45 divisions, *read* 4,5.



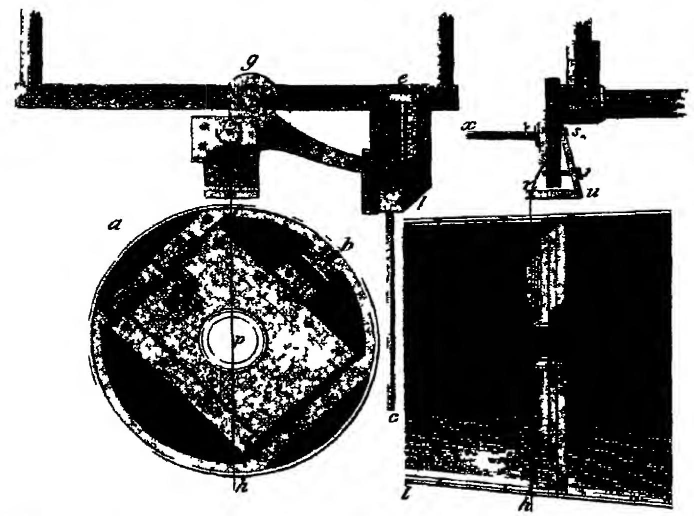



W
W
P
P
E
B
L
L
S
V
L
F
A

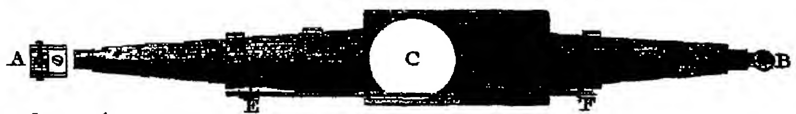
Section *of the Bottom of the* Telescope, *with its* Micrometer Screw



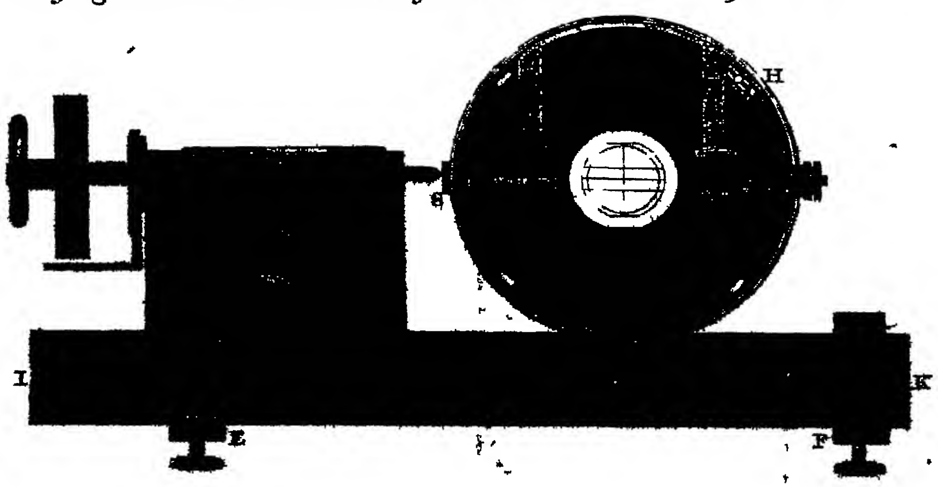
Section *of the* Diaphragm *carrying the* Dot
also a Section *of the* Axis *with the* Diaphragm



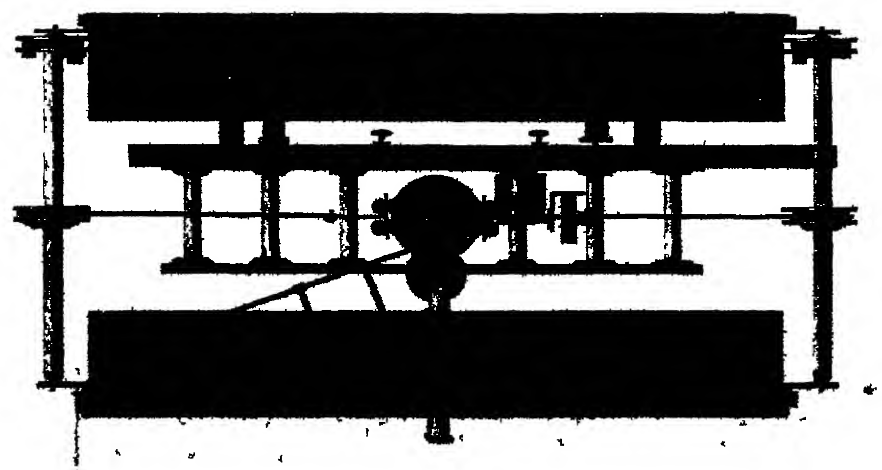
Horizontal View *of the Upper Part of the* Axis

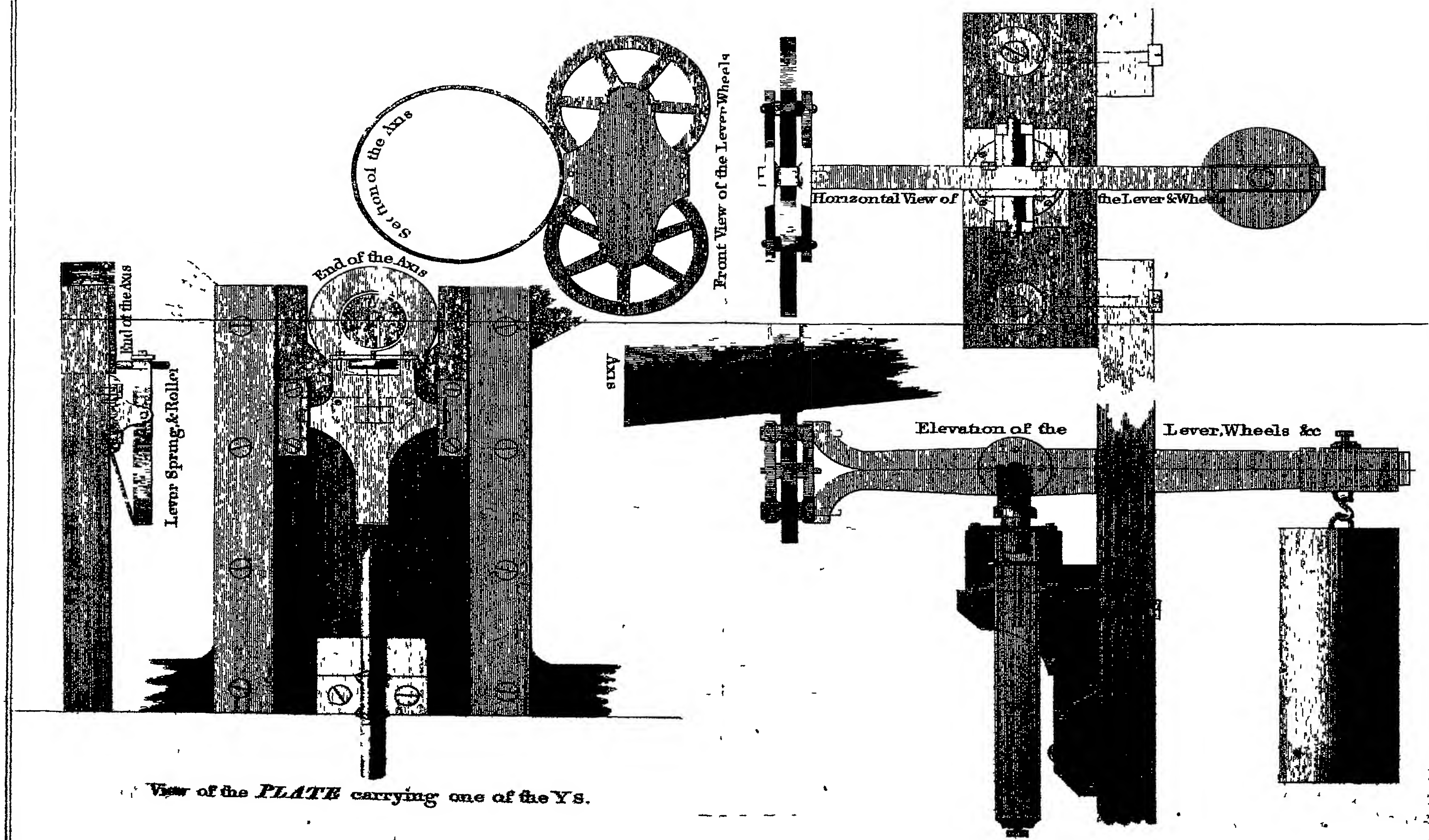


Horizontal View *of the End of the* Telescope, *with the Apparatus carrying the* Wires, *and also a view of its* Micrometer Screw



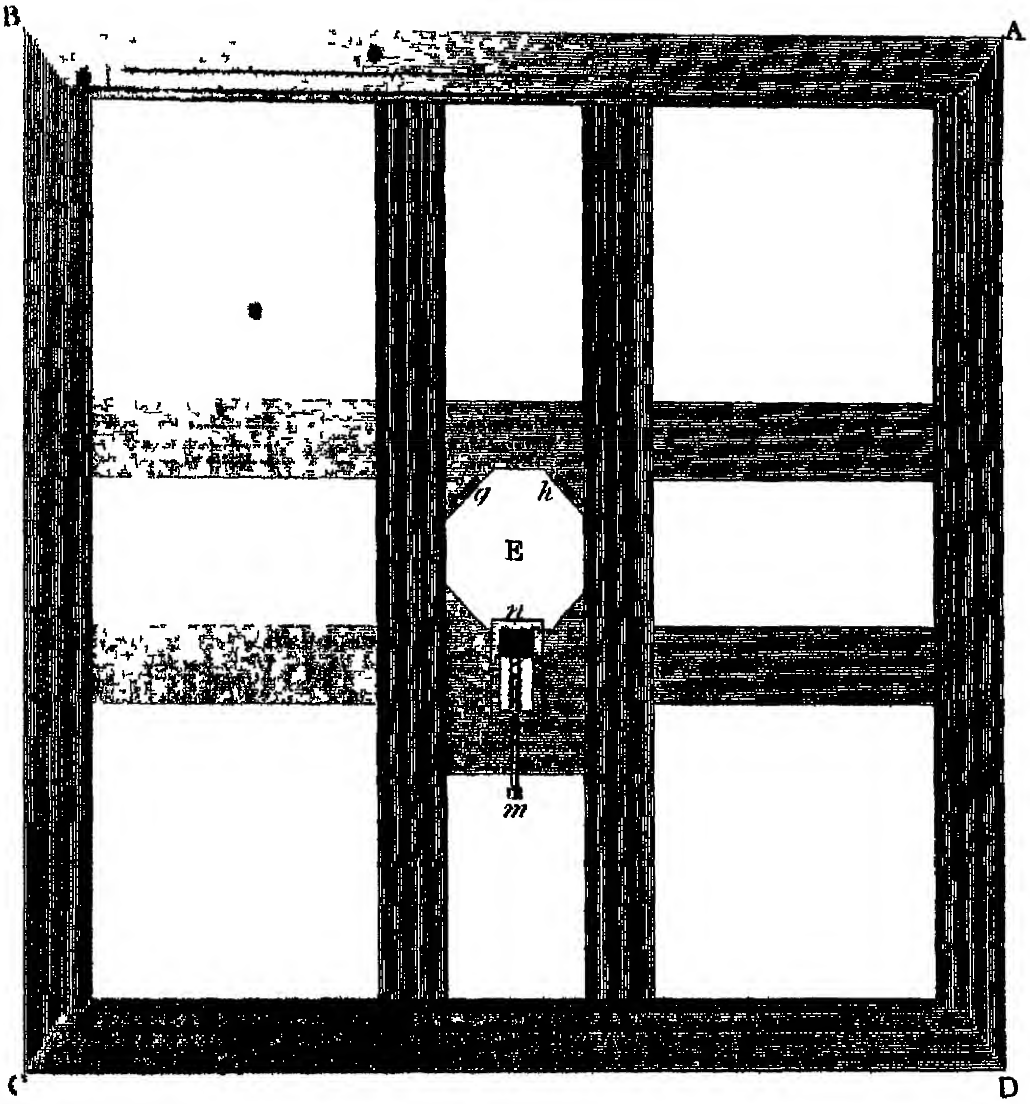
Horizontal View *of the* Axles, Pulleys *and* Arches



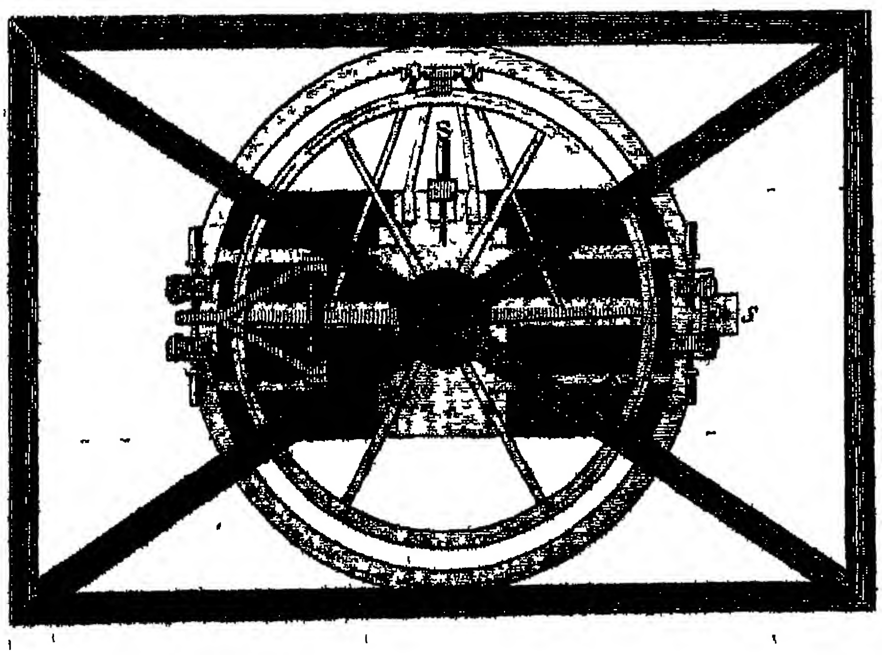


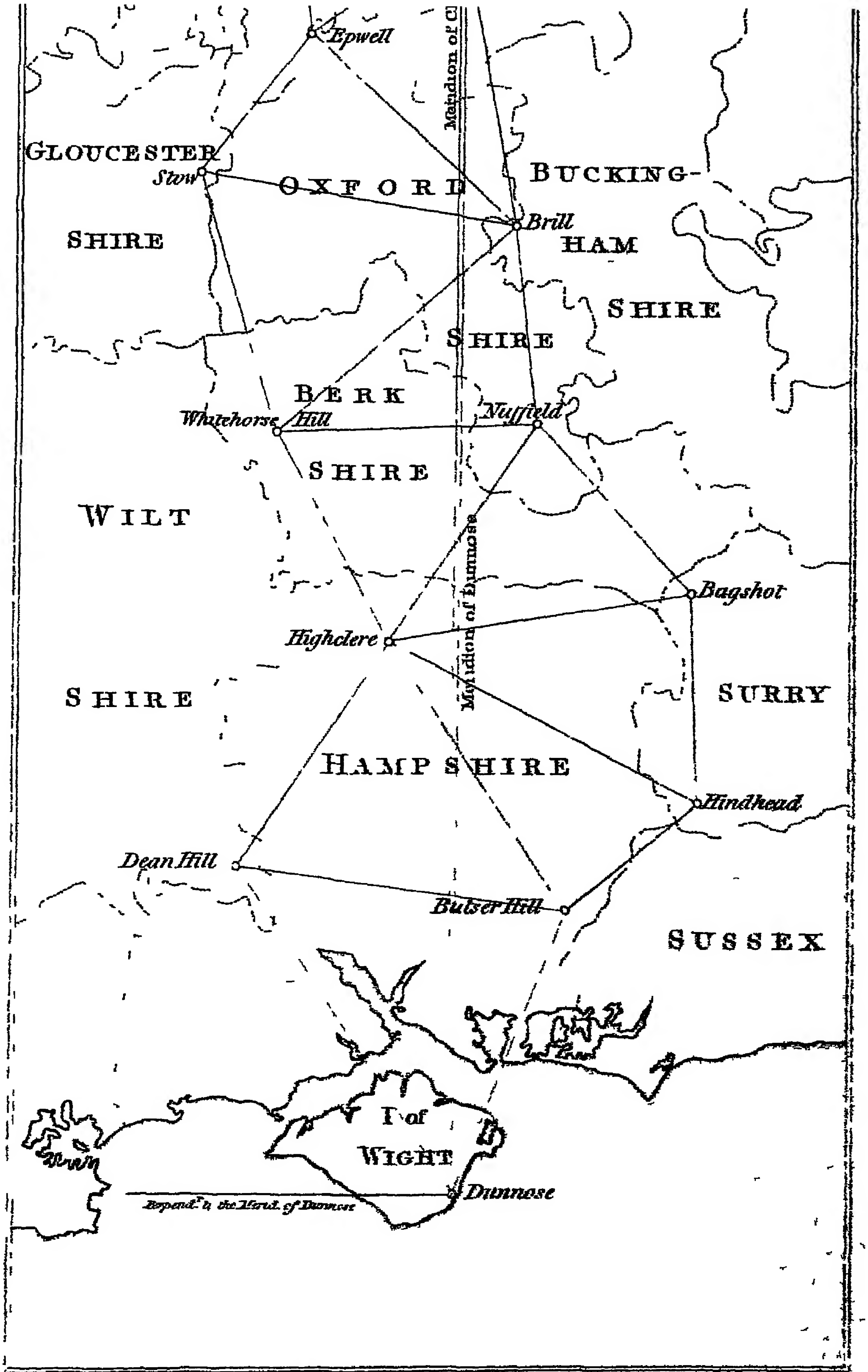
View of the PLATE carrying one of the Y S.

Top *of the External* Stand



ottom *of the Internal* Frame *resting on the* Azimuth Circle




Epwell
GLOUCESTER
Stow
OXFORD
BUCKING-
Brill
SHIRE
HAM
SHIRE
SHIRE
BERK
Whitehorse Hill
Nuffield
SHIRE
WILT
Bagshot
Highclere
SHIRE
SURRY
HAMPSHIRE
Hindhead
Dean Hill
Butser Hill
SUSSEX
I. of
WIGHT
Dunnose

TRIANGLES for ascertaining the Meridional Distance between CLIFTON and DUNNOSE

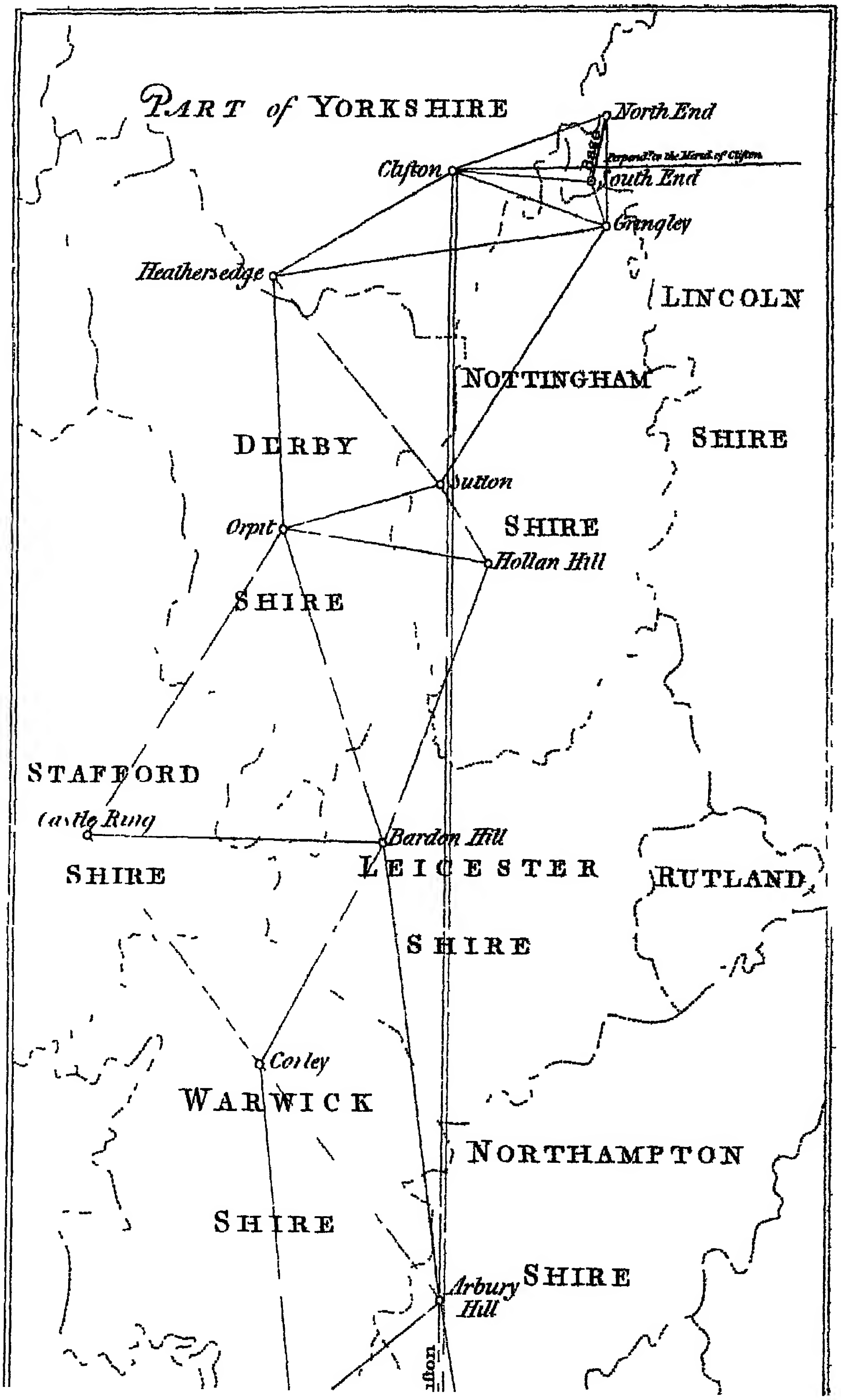


Fig. 1

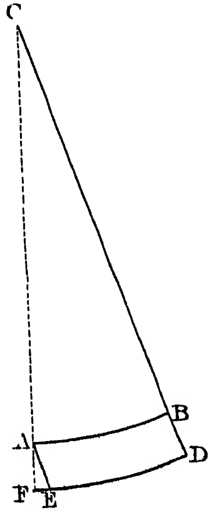
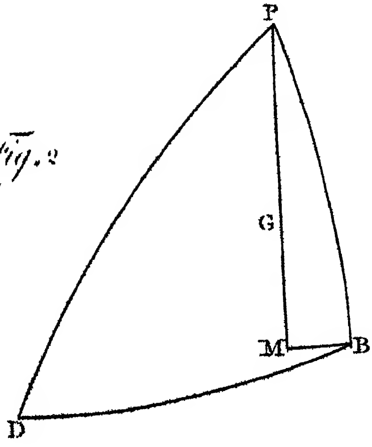


Fig. 2



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	Transactions of the Royal Society of Edinburgh. Vol. V. Part II. 1802. 4°	The Royal Society of Edinburgh.
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	Meteorological Journal kept on board the Marine Society's Ship, in the Years 1800 and 1801. MS. fol.	The Marine Society.
	Abstract of the Answers and Returns made pursuant to the Population Act. Ordered to be printed 21 Dec 1801. 2 Vols. fol.	Right Hon. Charles Abbot, F. R. S.
	Analytical Institutions, by Donna Maria G. Agnesi; translated by the late Rev. John Colson. London, 1801. 2 Vols. 4°	The Rev. John Hellins, B. D. F. R. S.
	Specimens of British Minerals, selected from the Cabinet of P. Rashleigh. The Second Part. London, 1802. 4°	Philip Rashleigh, Esq. F. R. S.
	A General Atlas, published by Robert Wilkinson: Europe, Asia, Africa, France.	John Wilkinson, M. D. F. R. S.
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	Beiträge zur chemischen Kenntniss der Mineralkörper, von M. H. Klaproth. 3 Band. Berlin, 1802. 8°	Professor Klaproth, F. R. S.
	Bibliotheque Britannique. No. 153—158.	Professor Pictet, F. R. S.

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Della Scoperta del nuovo Pianéta Cerere Ferdinandea. Palermo, 1802. 4°	M. Piazzi.
An Account of the English Colony in New South Wales, by Lieut. Col. Collins. Vol. II. London, 1802. 4°	Lieut. Colonel Collins.
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A Journal of Natural Philosophy, by W. Nicholson. No. 7—11.	Mr. William Nicholson.
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11. The Charter and Bye Laws of the Royal College of Surgeons in London. London, 1802. 8°	The Royal College of Surgeons.
Saggio di Esperienze sul Galvanismo, di Gio. Aldini. Bologna, 1802. 8°	Professor Aldini, of Bologna.
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Meteorological Journal kept at Cumberland House,	Joseph Colen, Esq.

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	from Oct. 14, 1778, to March 4th, 1779; and from Feb. 23 to June 3, 1790 MS. fol.	
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Dec. 16.	Carte du Canal royal de la Province de Languedoc, lévée par les Ordres des Etats Généraux de la dite Province. 1774 15 sheets	His Excellency the French Ambassador.
	Histoire du Canal du Midi, connu précédemment sous le Nom du Canal de Languedoc, par F. Andreossy. Paris, An. 8. 8°	————
	Campagne sur le Mein et la Rednitz, de l'Armée Gallo Batave aux Ordres du Général Augereau. Paris, 1802. 8°	————
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1803. *Jan.* 13.	A Journal of Natural Philosophy, by W. Nicholson. No. 13.	Mr. William Nicholson.
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	J. E. Bode von dem neuen achten Hauptplaneten des Sonnen Systems. Berlin, 1802. 8°	————
	Osservazioni sù i Punti fondamentali della Dottrina del Citt. Quatremere Disjonval, rapporto all'Origine delle Arti, de'Culti, del Linguaggio, e della Scrittura, del Citt. L. Bossi. Torino, An. 11. 8°	Sig. Luigi Bossi.

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